Medical Publications

HARVARD MEDICAL SCHOOL

1890

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PREFACE.

The following articles are presented in bound form to show the character of the original work done by the instructors of the school, or under their personal supervision. It was not thought desirable to include clinical papers in this collection.

J. COLLINS WARREN,
Chairman of the Committee on Publications.

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CONTENTS.

THE STERNUM AS AN INDEX OF SEX, HEIGHT, AND AGE.[1] By THOMAS DWIGHT, M.D., LL.D., *Parkman Professor of Anatomy at Harvard University, U.S.A.*

IN a short paper published in the *Journal of Anatomy and Physiology*, vol. xv., 1881, I gave the measurements of the sterna of 30 men and 26 women, and discussed the correctness of Hyrtl's statement, that it is hardly possible to err in determining the sex of this bone, and that "the manubrium of the female sternum exceeds half the length of the body, while the body in the male sternum is, at least, twice as long as the manubrium." This small series gave for the male a mean length of 5·18 cm. for the manubrium and 10·58 cm. for the body (total, 15·77 cm.), and for the female 4·67 cm. for the manubrium and 8·95 cm. for the body (total, 13·62). Thus the law was shown to hold good for the mean sternum in both sexes, but yet it failed to apply to 12 of the 30 men and to 14 of the 26 women.

In the same year Strauch presented an inaugural dissertation at Dorpat, which I know only through an abstract in *Hofmann and Schwalbe's Jahresberichte.* He measured 200 bones, apparently including the ensiform cartilage. The mean length of the manubrium is not given in the abstract, but we are told that in the male it is ·007 cm. shorter than in the female. The difference of length between the sexes is practically wholly in the body, which is on the average about 2 cm. longer in man (11 cm. and 9 cm.). Strauch found that the different parts of the male sternum are in general absolutely thicker and broader but relatively thinner and narrower than in the female. In male and female sterna of equal length the former has a narrower manubrium, and the latter greater breadth at the lower part of the body. He found that in man the length of the

[1] From a paper entitled "Medico-Legal Studies on the Human Skeleton," read December 27, 1889, at the meeting of the Association of American Anatomists at Philadelphia.

manubrium is to that of the body as 1 to 2·1, and in woman as 1 to 1·7, thus again confirming Hyrtl's law for the mean.

TABLE I.—*Length.*

Cm.	Manubrium.		Body.		Total.	
	Men.	Women.	Men.	Women.	Men.	Women.
2·5	1	...	...	...	...	...
3·	1	...	...	...	...	...
3·5	3	2	...	...	...	...
4·	9	16	...	...	...	...
4·5	15	21	...	...	...	...
5·	49	29	2	...	...	...
5·5	35	14	...	...	...	...
6·	20	3	...	...	...	...
6·5	4	1	...	2	...	...
7·	3	...	1	1	...	...
7·5	1	...	...	8	...	...
8·	...	...	...	10	...	...
8·5	1	...	2	14	...	...
9·	...	...	9	18	...	...
9·5	...	...	6	14	...	...
10·	...	...	33	7	1	...
10·5	...	...	17	5	...	...
11·	...	...	22	4	1	2
11·5	...	...	15	2	...	2
12·	...	...	13	...	...	5
12·5	...	...	10	1	3	4
13·	...	...	2	...	...	12
13·5	...	...	2	...	1	15
14·	...	...	3	...	8	13
14·5	...	...	1	...	8	9
15·	...	...	2	...	17	11
15·5	...	...	1	...	15	5
16·	...	...	...	...	22	3
16·5	...	...	1	...	17	2
17·	...	...	...	...	14	1
17·5	...	...	...	...	18	2
18·	...	...	...	...	4	...
18·5	...	...	...	...	1	...
19·	...	...	...	...	7	...
19·5	...	...	...	...	...	...
20·	...	...	...	...	2	...
20·5	...	...	...	...	1	...
21·	...	...	...	...	1	...
21·5	...	...	...	...	...	...
22·	...	...	...	...	...	...
22·5	...	...	...	...	1	...
Total,	142	86	142	86	142	86

The series I now present includes the one already published in this *Journal*. It represents measurements of 228 sterna,

of which 142 are male and 86 female. The means are as follow:—

	Manubrium.	Body.	Total.
Male, . .	5·37 cm.	11·04 cm.	16·41 cm.
Female, .	4·94 ,,	9·19 ,,	14·13 ,,

Once more, we find that the proportions of the mean sternum follow Hyrtl's law, but a study of the individual cases shows that among the men 59·1 per cent. agree with it and 40·8 per cent. do not; while in women 60·4 per cent. agree and 39·5 per cent. do not. We conclude, therefore, that the law does not apply to two persons out of five, and thus can be of no value in the case of an individual.

It is best not to be content with taking an average, but to divide the sterna and their parts into groups of certain lengths, from which we may gain a more just idea both of the mean and of the range of variation. Table I. shows the number of bones, and of parts of bones, in each group, which includes all of the length indicated, and shorter than those of the next group.

This table also shows that the variation in the length of the manubrium is very slight. Of the male ones 119 are from 4·5 cm. to 6·5 cm., and all but 6 of the female from 4 cm. to 6 cm. The bodies vary more; still 110 male ones range only from 10 to 13 cm., and of the female 56 from 8 to 10 cm. The variation of the different parts, and consequently of the totals, is greater among men than among women, but the larger number in the male series must be kept in mind. The smallest sternum is a male one.

The Relation of the Length of the Sternum to the Height.—Strauch found that the length of the sternum (the ensiform being, no doubt, included) was $\frac{1}{7 \cdot 78}$ of the height in man and $\frac{1}{8 \cdot 04}$ in woman, but that there was no constant relation between the length of the bone and the height, as Körber[1] apparently had maintained. I know absolutely nothing further of Körber's observations. I have studied this question on 70 men and 39 women, all of whom were white. (There are a few observations on negroes in the preceding table.) The results are in some respects very remarkable. As a matter of course the average

[1] *St Petersburg Med. Zeitschrift*, iii. 2.

length of the parts of the sternum is not precisely the same as in the larger series. The first line of the following table shows the average height of 70 men, the length of the manubrium and of the body of the sternum, and the relation of the sum of these to the height taken as 100. I then divided the group into two equal ones of the 35 shortest and the 35 tallest respectively. In the shorter group the height ranges from 155·5 cm. to 169·5 inclusive, and in the taller from 169·6 cm. to 191·5 inclusive. I then picked out three groups—one of the 11 shortest men, ranging from 155·5 cm. to 163·7 ; one of the 13 tallest, from 176·1 cm. to 191·5; and a third group of 23 men from the middle of the series whose heights were very nearly equal, as they ranged only from 168 cm. to 170·8 cm. (all inclusive). Twelve of this group belonged to the shorter half and 11 to the taller.

TABLE II.—*Men.*

Group.	Height.	Manubrium.	Body.	Total.	Percentage of Height.
70	169·9 cm.	5·5 cm.	10·9 cm.	16·4 cm.	9·65 per cent.
35 shortest	165·1 ,,	5·3 ,,	10·8 ,,	16·1 ,,	9·75 ,,
35 tallest	174·7 ,,	5·7 ,,	11·06,,	16·8 ,,	9·61 ,,
11 shortest	160·3 ,,	5·4 ,,	10·5 ,,	16·– ,,	9·98 ,,
23 middle	169·4 ,,	5·3 ,,	10·9 ,,	16·2 ,,	9·56 ,,
13 tallest	179·7 ,,	5·7 ,,	11·4 ,,	17·2–,,	9·57 ,,

We see that the total length of the sternum increases with the height in every group in due order, and that the same is true of the body, while the manubrium shows some variation. It is important to notice, however, that the proportion to the height varies in different groups much less than one would expect, knowing the range of individual variation. The variation in the percentage of the height formed by the sternum is, I think, surprisingly small. As might be expected, the percentage is larger in the shorter bodies than in the taller, though the table does not show a perfect regularity. The manubrium shows a general tendency to increase with the height.

I divided the 39 women into two groups of the 19 shortest and the 20 tallest, ranging respectively from 144 cm. to 156·7 cm., and from 157·2 to 171·2, all inclusive.

TABLE III.—*Women.*

Group.	Height.	Manubrium.	Body.	Total.	Percentage of Height.
39	156·1 cm.	5·03 – cm.	9·3 cm.	14·4 – cm.	9·22 per cent.
19 shortest	151·5 ,,	5·02 ,,	8·9 – ,,	13·9 ,,	9·17 ,,
20 tallest	160·4 ,,	5·03 ,,	9·8 ,,	14·85 ,,	9·26 ,,

The most striking thing in this table is the constancy of the length of the manubrium in all the groups. The next is that, while among women the sternum is shorter in proportion to the height than in men, it is relatively smaller in the shorter women than in the taller ones. I cannot but think, however, that this should be received with caution till the point is settled by a larger series. Curiously enough, the smallest percentage of the height is found in the sterna of the group of the 19 shortest women (9·17 per cent.) and the greatest in those of the group of the 11 shortest men (9·98 per cent.). Thus the extreme variation between all the groups is only ·81 of 1 per cent.

We must now consider the variation among the individuals of the different groups. In that of the 11 shortest men the lowest percentage is 8·95 and the highest 11·06, but the next lowest is 9·24 and the next highest 10·67. The mean is 9·98. If we exclude the two extreme individuals, the variation is not great.

The mean of the 13 tallest men is 9·57, and the extreme limits 8·66 and 10·31.

The middle group of 23, with a mean percentage of 9·56 per cent., presents extreme variations of 8·41 per cent. and 10·60 per cent. Thirteen of the 23 are between 9 per cent. and 10 per cent.

Of the 35 shortest men, the extremes are 6·80 per cent. and 11·57 per cent. The former is, however, a very exceptional case, in which the combined length of the manubrium and body is only 11·4 cm. The next lowest is 8·41 per cent. There are ten below 9·25 per cent., of which two are 9·22 per cent. and 9·25 per cent. respectively. Nine are above 10·25 per

cent. Thus the mean being 9·75 per cent., 16 are within one-half of 1 per cent. of it.

Of the 35 tallest men, the smallest percentage is 8·58 per cent. and the largest 11·19 per cent. The latter is an uncommon case, the sternum being 19·22 cm. long. The next highest percentage is 10·91 per cent. The mean is 9·61 per cent.

The smallest percentage of the 19 shortest women is 8·01 per cent. and the greatest 10·35 per cent., and the next greatest 9·83 per cent. The mean is 9·17 per cent. Only three are more than half of 1 per cent. below it, and only six more than as much above it. Of the latter, two pass the limit by only ·01 of 1 per cent.

The mean of the 20 tallest women is 9·26 per cent. The smallest is 7·27 per cent. and the next 8·18 per cent. The two highest are 11·38 per cent. and 10·49 per cent. Five are more than a half of 1 per cent. below the mean, of which two are near the line, and five are more than as much above it.

It would be necessary to exclude but a few cases to show a remarkably small individual variation from the means in the different groups. Certainly, as with Hyrtl's law, one would not be justified in attaching great weight to this guide to the height in a single case; but it seems to me probably as trustworthy a basis of estimation as the long bones, and perhaps even a better. I hope, at least, that some others may think it worth while to add to the number of observations.

The Ossification of the Sternum.—In my previous paper I expressed the opinion that the sternum is of little value as an index of age. I could remember, I stated, but very few instances of persons of 20 or over, in which the body was not one piece. This is, of course, in opposition to the entirely imaginary table still to be found in some standard works, according to which the third piece of the sternum (counting the manubrium as the first) joins the fourth at from 20 to 25, and the second at from 35 to 40. Further observation entirely confirms my views. I cannot even agree with some high authorities who state that the second piece often does not join the rest of the body till 25. I must add, however, that from a few observations, I incline to the opinion that the

several parts of the body unite later in the lower races. The dates of the union of the body with the ensiform, and of its less frequent union with the manubrium, are most uncertain. The following is a short statement of my observations on 46 male and 26 female bodies. The youngest was a girl of 17; in her case there was a broad strip of cartilage between the manubrium and the body. The first piece of the body was distinct from the rest, which was one piece of bone, showing a faint transverse line on its front surface at the junction of the second and third pieces of the body. There was no bone in the ensiform. This sternum is the only one in this series in which the body is not one piece. Unfortunately, there are no others younger than 25.

The Male Sterna.—These are 46 in number. The manubrium, body, and ensiform are all distinct in 18 cases of the following ages:—28, 31, 35, 37, 37, 38, 40, 41, 43, 43, 49, 54, 55, 62, 65, 68, 71, and 79.

The manubrium is distinct, and the body and ensiform are joined in 13 cases, aged—28, 31, 35, 37, 37, 39, 45 (black), 47, 55, 66, 70, 72, and 75. To this group may be added five cases, aged 51, 65, 68, 75, and 82 (?), in which the ensiform seems on the point of joining the body, or in which it is doubtful whether they are really united or not.

The manubrium, body, and ensiform are all co-ossified in seven cases. In the youngest, 25, the union of the body and ensiform had just begun. In two of 36 and 38 the union between the manubrium and body is only beginning. A man of 43 had extensive anchylosis of the spine. In one of 44 the union of the manubrium was just beginning, and that of the ensiform doubtful. One of 45 requires no comment. In one of 65 the union between the manubrium and body was slight.

The following three cases may be put together. In a man of 38 there was slight union between the manubrium and the body, the ensiform being distinct. In a negro of 42, the manubrium and body were partly co-ossified, while the ensiform was free and contained no bone. In the sternum of a white man of the same age, the manubrium and body formed but one piece of bone, while the ensiform, which contained little bone was free. This sternum is so remarkable, owing to the apparent

prolongation of the manubrium to the third ribs, that it shall form the subject of a separate paper.

The Female Sterna.—The examination of 26 cases results as follows:—the manubrium, body, and ensiform are all distinct in 12 cases, aged as follows: 17 (described above), 26, 26, 31, 32, 33, 45, 50, 50, 60 (very little bone in ensiform), 65 and 80. To these may be added one of 39, of which it was recorded that the ensiform was probably distinct.

The manubrium was found distinct, and the body and ensiform joined in eight subjects of the following ages: 40, 44, 56, 60 (?), 64, 67, 82, and 91. The manubrium, body, and ensiform are all united in a specimen from a woman of 51.

The manubrium and body are united and the ensiform still distinct at 26 and 70. In a case aged 51 the manubrium and body were just beginning to join, the ensiform being still distinct.

The sternum of a negress of 38, which probably belongs in the first group, is sufficiently curious to deserve a special description. The manubrium and body are still distinct. The body, as seen from the front, ends at the level of the insertion of the fourth pair of costal cartilages. The fifth pair are attached to its lower end. The fifth, sixth, and seventh pairs of cartilages meet one another and their fellows of the opposite side, forming a cartilaginous continuation of the sternum. There is some calcification of the fifth pair at the sternum. Seen from behind, the body appears a little longer, a thin layer of ossification extending down between the fifth cartilages. The ensiform cartilage was almost wholly cut away. What little is left contains no bone, and appears to spring from the deep surface of the united costal cartilages. The specimen is a very pretty demonstration of Ruge's views of the development of the sternum from the costal cartilages.

These tables show that the body of the sternum is but one piece of bone at 25 years.[1] Probably a larger series would show

[1] Since reading this paper I have met with the sternum of a white man aged 46, in which union between the first and second pieces of the meso-sternum had evidently but just begun. The manubrium and ensiform were still free. I have also examined the bodies of a white woman of 20, of a negro of 20, and of a white man, an Armenian, of 21; in all the meso-sternum was in one piece.

some few exceptions, but I think it would show also that the bone is usually in this condition some years earlier. This belief is founded on scattered observations which could not be included in this series. These tables show also how utterly uncertain is the time of union of the manubrium and ensiform with the body of the sternum.

IRREGULAR UNION OF THE FIRST AND SECOND PIECES OF THE STERNUM IN MAN AND APES.

By THOMAS DWIGHT, M.D., LL.D., *Parkman Professor of Anatomy at Harvard University, U.S.A.*

AMONG the breast-bones shown at the reading of the preceding paper at Philadelphia was a very remarkable one, which must be discussed in a separate article. It came from the body of a white man forty-two years old. His height was 164·6 cm., and the length of the bone (without the ensiform) is 16·5 cm. The peculiarity of this bone (fig. 1), consists in the

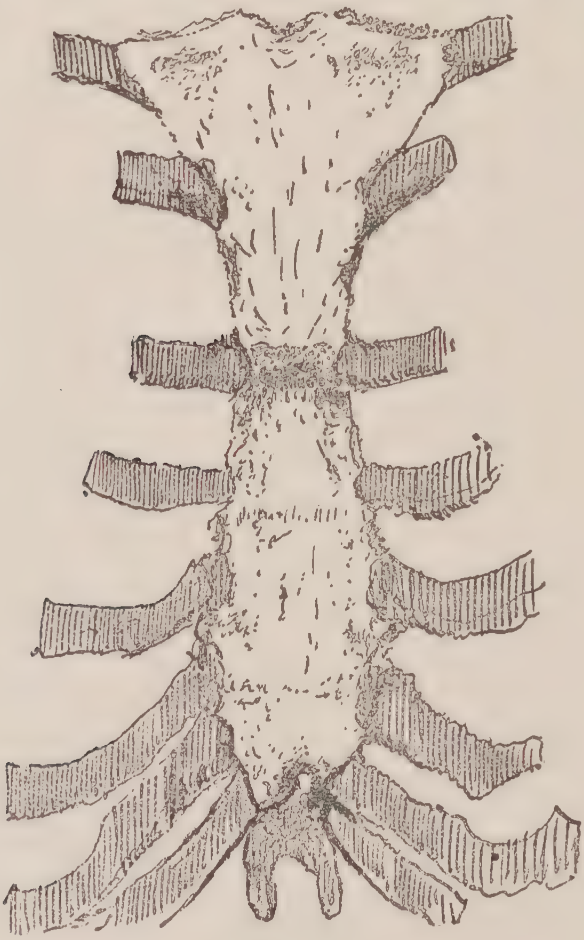


Fig. 1.

apparent continuation of the manubrium to the level of the third costal cartilages. It gradually narrows to that level, and there presents a strong transverse prominence which must, have been felt with great distinctness through the skin, being

I think, greater than the usual one at the lower border of the manubrium. There is a much slighter prominence at the same place on the back of the bone. The second ribs are very near the first, and the portion of the sternum above their level very short. It is only by very careful inspection that any inequality of the surface is seen on the front of the sternum between their ends, but an almost imperceptible thickening of the bone may be detected by the finger. There is no sign of a suture. The ensiform is separate, but the rest of the sternum is one piece of bone. Eight costal cartilages join it on the right. On the left the eighth just fails to reach it. The apparent manubrium forms nearly half the length of the bone.

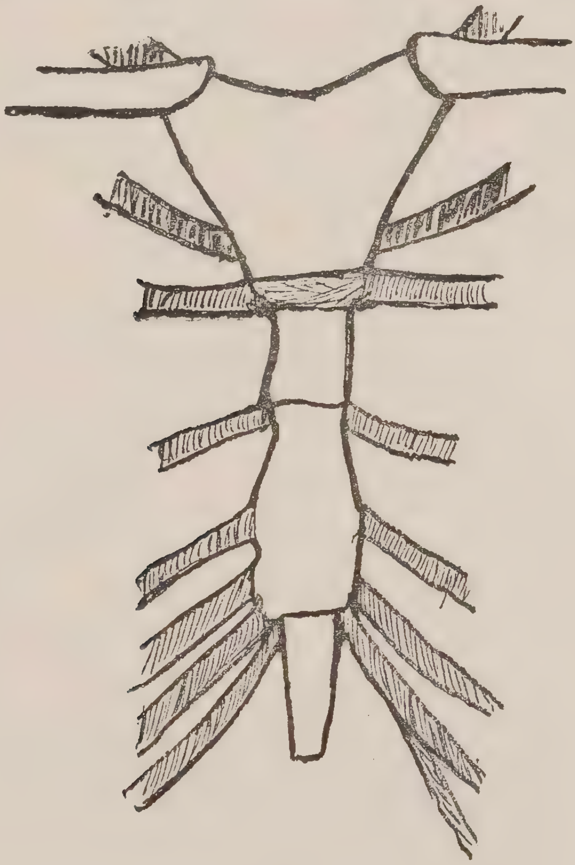


Fig. 2.

Dr Lamb, of the Army Medical Museum at Washington, expressed the opinion that all the bone above the third cartilages should be considered as the manubrium, and said that he thought this condition exists in some monkeys. On my return to Boston, I examined the skeletons in the Museum of the Society of Natural History, and looked through several works on comparative anatomy, without finding anything in support

of Dr Lamb's views. After a time, however, I received a letter from him, in which he stated that he had found this condition in the *Hylobates leuciscus*, and he informed me later that a diagram which he sent me of the sternum of this animal would do very well for that of the *Hylobates lar* also. I then visited the Museum of Comparative Zoology at Cambridge (Mass.), and found a skeleton of each of these animals. Fig. 2 represents rather diagramatically the sternum of *Hylobates leuciscus.* The third ribs end against a strip of cartilage separating the manubrium from the next piece. The second ribs are but very little higher. Dr Lamb's diagram differs from mine chiefly in the point of insertion of the second ribs, which is about midway between those of the first and third ribs. The ossification of the meso-sternum is the same in both. The *Hylobates lar* at Cambridge is quite different. The only peculiarity is that the second costal cartilages touch the body of the sternum only by their lower edges resting almost wholly against the manubrium. There is no intervening strip of cartilage.

I am further indebted to Dr Lamb for calling my attention to a series of lectures by de Blainville, translated and edited by Dr Robert Knox, published in the *Lancet* of 1839–40. From these lectures I would quote the following passage about the Gibbons:—[1] "The sternum is large, short, approaching the human in form, and composed, in the adult skeleton at least, of three large portions—an anterior, which represents two very unequal sternebræ, and which in fact, supports two pairs of ribs, a central part of moderate dimensions, which corresponds to three or four sternebræ in the Callitriche, and with which, in fact, are articulated the other five pairs of sternal ribs ; lastly, a third portion (xiphoid) of considerable length placed between the united cartilages of the asternal ribs." This is illustrated by the cut which is reproduced as fig. 3. It was taken from de Blainville's *Atlas*,[2] and represents the sternum of *Pithecus syndactylus*, now *Hylobates syndactylus.* It shows the meso-sternum bearing four pairs of ribs instead of five, according to the description just quoted. Dr Knox published in the same volume of the *Lancet* a memoir on the *Gibbon varié*, with a

[1] *Lancet*, 1839–40, vol. ii. p. 212.
[2] Vol. iv. plate viii.

critical examination of de Blainville's account of the gibbon. Dr Knox's observations are based on two gibbons which he had received undissected. One was an adult female, the other a young one. Of the former he wrote:—"The thoracic sternebræ are three in number; and in this respect, but more particularly in the flattened broad form of these portions, the sternum resembles the human sternum so closely as, we imagine, would deceive most anatomists. There are distinctly seven pairs of sterno-costal cartilages, and these are rather curiously placed, not being at equal distances from each other. The second and third pairs approach each other very closely, and would seem to

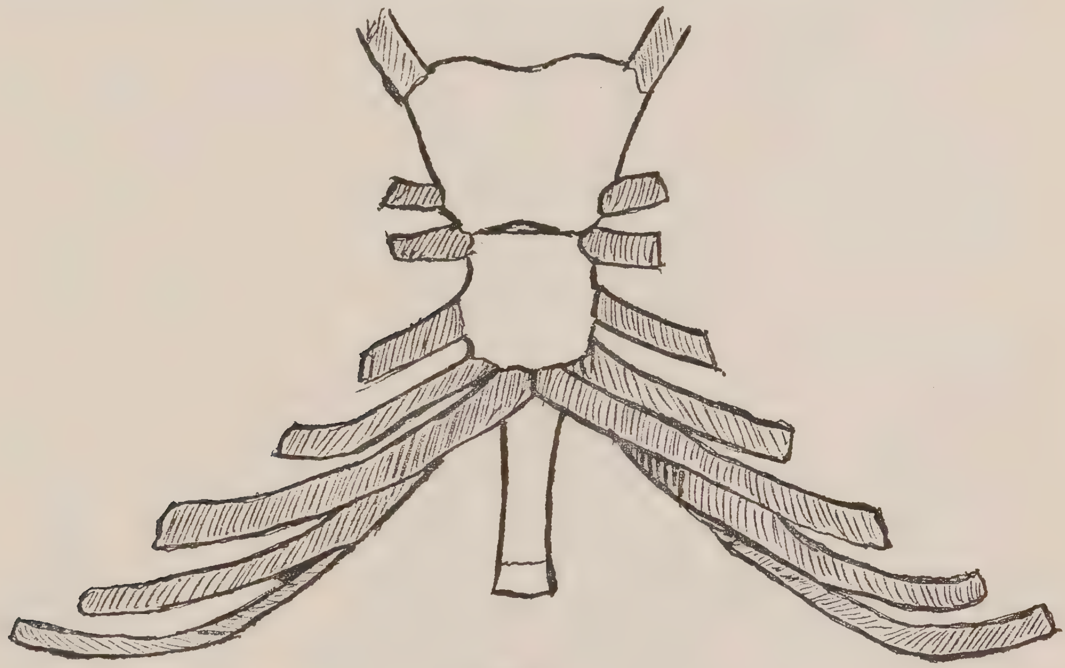


Fig. 3.

have originally divided, as it were, the manubrium from the body, *i.e.*, the first from the second sternebra. But we will immediately show, from the examination of the young gibbon's sternum, that this is not the case, and that a small sternebra is developed, but unites with the manubrium as the animal approaches maturity." Of the young one he wrote:—"The sternebræ are in an interesting period of their development. We have six separate and distinct centres of ossification, forming six sternebræ; the second uniting with the manubrium or first, and the third, fourth, and fifth uniting together will precisely give the arrangement of the adult sternum." It should be observed, however, that this does not give any evidence that

these pieces would, in point of fact, have fused in this way had the animal lived longer. I owe also to Dr Lamb's kindness a reference to Duvernoy[1] who states that in the *syndactylus* the first two pairs of ribs join the first piece of the sternum, and the third pair the first and second pieces as the second pair does in other genera.

Further researches among the writings of anatomists which I undertook yielded very little. In a French edition of Meckel's *Comparative Anatomy*, 1829, I find it stated that the first piece of the sternum of the hippopotamus gives attach-

Fig. 4.

ment to three pairs of ribs, and the author continues—"It is the same with the gibbon: the first piece is relatively very large, and is in relation with the first, second, and third pairs of ribs: but then there comes another piece, a small one, situated between the third and fourth pairs of ribs, and only after that a larger piece which receives the ribs from the fourth to the seventh." It is curious to note that the progress of fusion of the different pieces of the sternum corresponds precisely with

[1] *Archives des Muséum d'Histoire Naturelle*, 1855–6, vol. viii. p. 24.

that shown in fig. 2 representing the sternum of the *Hylobates leuciscus* in the Harvard Museum, and agreeing with Dr Lamb's diagram of the *Hylobates leuciscus* and the *Hylobates lar* at Washington, but not with the *Hylobates lar* at Cambridge.

Owen[1] says briefly, that "two pairs of ribs and a part of a third pair articulate with the manubrium." It is not quite clear to me whether this statement refers to gibbons in general or only to *Hylobates leuciscus.*

Dr Giebel[2] writes rather vaguely that the sternum of *Hylobates syndactylus* receives on each side the first two pairs of ribs one after the other. Of the *Hylobates leuciscus* he says only that its sternum is narrower than that of the chimpanzee and ourang. He begins by saying that the species of *Hylobates* vary considerably among themselves (in the matter of the sternum).

There seems to me no doubt of the last statement, and indeed very little doubt that individual gibbons vary greatly, for in no other way can I account for the silence of many high authorities on this interesting point.

In the gorilla it sometimes happens that the first piece of the meso-sternum fuses with the manubrium, while it is distinct from the piece below it. I have seen two such cases, one in Boston and one in Cambridge, and Professor Mivart[3] refers to a similar condition in the large gorilla in the British Museum.

The following observations are the only ones I know of in human anatomy. The first is by Meckel,[4] which I owe to a reference in Humphry's *Human Skeleton.* The sternum was that of a man. He states briefly, that as usual it consisted of three pieces, only the first was relatively much larger than usual, and occupied not only the space between the first and second ribs, but that between the first and the third pair.

Sir William Turner[5] mentions an Andaman Islander in whom the manubrium was fused with the first segment of the meso-sternum before the meso-sternum itself had completed its ossification, but this, he adds, "is quite exceptional."

[1] *Anatomy of Vertebrates*, vol. ii. p. 520.

[2] Bronn's *Klassen und Ordnungen des Thier-Reichs.*

[3] "Axial Skeleton of the Primates," *Proc. Zool. Soc.*, 1865, p. 567.

[4] Meckel's *Archiv*, B. iv. 1818.

[5] *Challenger Reports*, "Human Skeletons," vol. xvi. p. 78.

In the report in this *Journal*[1] by the writer of the case of a male Western Islander, aged twenty, who had cervical ribs and peculiarities of the spine, it was stated that "the first piece of the sternum was prolonged upward, so that the articular surface for the clavicle looked almost directly outward. The manubrium and the second piece were firmly grown together, but the others were all distinct."

The most remarkable feature of the present case, is not the fact that the piece which should have been the first of the meso-sternum evidently joined the piece above much earlier than the one below it, but that the angle on the front of the sternum is transferred to the line of the third ribs, that the second ribs are displaced upward, and that all above the angle has a very plain resemblance to a manubrium. On comparing the drawing of this sternum with those of the gibbons, we see that in the latter the second ribs approach the third, so that if we suppose there were originally two segments of the sternum above the third ribs, in the case of this man, the first is the one which is dwarfed, and in the gibbon it is the second. In my opinion, however, it would be quite unwarranted to assume that there is any connection of cause and effect between the peculiar character of the breast-bone of the gibbons and that of this human one. It probably was the result of what, in want of a better word, we may call accident. It would be interesting to know whether the surmise is correct, that individual gibbons vary greatly in this respect, and to ascertain what may have been the cause of the peculiarity which their breast-bones at least frequently present.

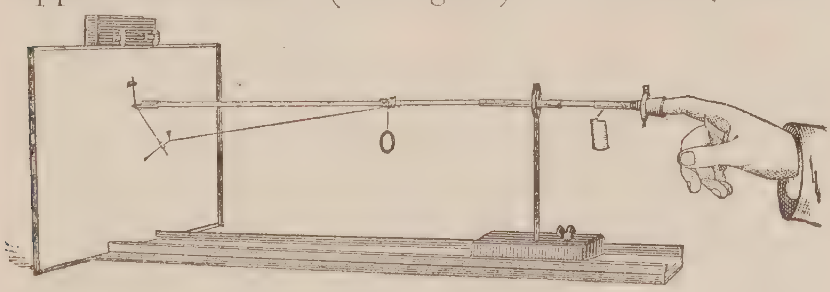
[1] Vol. xxi. p. 543.

OBSERVATIONS ON THE STEADINESS OF THE HAND AND ON STATIC EQUILIBRIUM.

BY W. N. BULLARD, M.D., AND E. G. BRACKETT, M.D.

In the spring of 1886, in conversation with Dr. H. P. Bowditch, one of us suggested the advisability of some contrivance by means of which the steadiness of the hand could be readily determined. Dr. Bowditch thought well of the matter, and kindly invented the instrument used, and had it made at the physiological laboratory of the Harvard Medical School. For this instrument we suggest the name of tremograph.

It consists essentially of a long, light, wooden rod, which runs through a closely fitting brass cylinder so hung in a vertical compass-joint on the top of a firm upright as to permit of movement both vertically and horizontally in all directions. One end of this rod is furnished with a thimble or some other contrivance for holding the finger, while to the other is attached a Pflüger's pen. Various weights may be attached to the rod at various distances from its point of support, which of course can be varied at will, so as to keep it when unmoved in an absolutely horizontal position. The upright, made of brass, is placed in the centre of a rectangular wooden base, which can be moved backward or forward on a wooden platform with low, grooved sides, into which it fits closely. At one end of this platform is a transverse groove behind which rises a transverse board. This groove serves to hold the glass plate over which the paper for the tracings is stretched, while the board supports it behind (see figure). To the contriv-



ance holding the pen is attached a string by which the whole can be pulled backwards when not in use, and this string may be fastened to a little hook on the rod. When in use this string is loosened, the penholder hangs vertically, and the pen, projecting horizontally, comes into contact with a sheet of paper, which is placed over a layer of glass so as to offer a surface both smooth and firm for writing. The finger of the person to be tested is then placed in the thimble, which is connected with the rod by a compass-joint, and the motions of the finger will be accurately recorded on the paper. According as the point of support of the rod is at its middle or nearer one or the other end, so will the lines on the paper be of the same size as the actual movement or proportionately greater or less. As a matter of fact the tracings were magnified four times, that is, they were to the movements of the hand as four to one.

The first experiments with this instrument were made in the spring of 1886, but they were too few to be of any positive value. The work was resumed in the winter and spring of 1887, and has been continued more or less steadily ever since.

In order to give our investigations a positive value, it was thought best to first determine what position of the hand was most favorable for the tracing of the tremors or movements to be studied. For this purpose the first series of tracings were made. They were used only to compare the results obtained according as the hand was held in different positions. The tracings in this series were taken from eighty-five patients at the Boston Dispensary, and six tracings were taken from each patient.

It is evident that from such subjects it would be impossible to obtain a normal standard of steadiness; for although all due care was taken not to make use of any whose disease would directly affect them in this regard, yet any person suffering from an ailment severe enough to cause him to seek medical advice could hardly be expected to produce as firm a tracing as in perfect health. Besides, not only did the ordinary nervousness of patients at such a time affect the subjects, but this was greatly increased by their apprehension at the sight of a new and unknown instrument, which was frequently

regarded as some modification of the electric battery, and momentarily expected to break out in some startling way.

For this reason it was early determined to use these tracings only to determine the relative value of the different positions of the hand for our purpose. The hand was tested in three positions in every case — pronation, supination, and midway between the two, both with the eyes open and with the eyes closed. This makes six different tests in each case, which for convenience have been numbered in the diagrams as follows: —

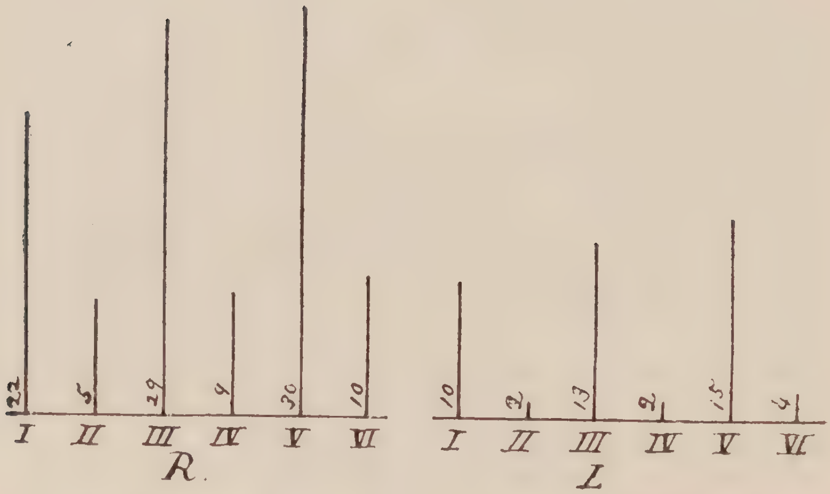
I. Pronation, eyes open.
II. Pronation, eyes closed.
III. Supination, eyes open.
IV. Supination, eyes closed.
V. Midway, eyes open.
VI. Midway, eyes closed.

All the tracings taken have been divided into two groups. I. Stable. II. Mobile.

In a certain proportion of cases no definite direction was taken, but the hand remained approximately at the point of starting. The tracing then appeared as an aggregation of many intercrossing lines, and was not developed in any special direction, its size, of course, varying directly with the steadiness of the finger. These cases have been designated as *stable.* In all the others the hand moved in some general direction from the starting point, and these have been grouped according to the direction taken by the hand (the opposite of that taken by the pen), under the headings Up, Down, Right, and Left.

Group I. Stable. The hand was held more or less firmly in the original position. It may or may not have been steady, but what movement there was was confined within very narrow limits, and there was no definite continuous movement in any one special direction.

The relative number of tracings of this character obtained under each of the six conditions is represented by the diagrams below. The two series represent respectively the numbers obtained when the right hand of the subject was tested, and when the left hand was used. In each series the height of the lines corresponds more or less roughly with the actual number of tracings in said series, and this number is noted by the Arabic figures, while the Roman numerals below each vertical line denote the special condition under which the tracing a s taken.



Comparing the lines in each diagram, which represent the number of the tracings in which the subject's eyes were open (Nos. I., III., V.), it is seen that the lines increase in height in the order named, and again, in those representing the number of tracings in which the eyes were closed (Nos. II., IV., VI.), a similar increase in the same order is observed. Thus the number holding the hand approximately in the original place is smallest in the position of pronation, and greatest when the hand is held midway between supination and pronation. Moreover, if we compare the average excursion, in both vertical and horizontal directions, of tracings in this group in which the eyes were open, with those in which the eyes were closed, the hand being held in each case in the same position, we find that the loss of the control of sight is felt more in the position of pronation than in either of the others, except in that of supination with the eyes closed, when the lateral movement predominated.

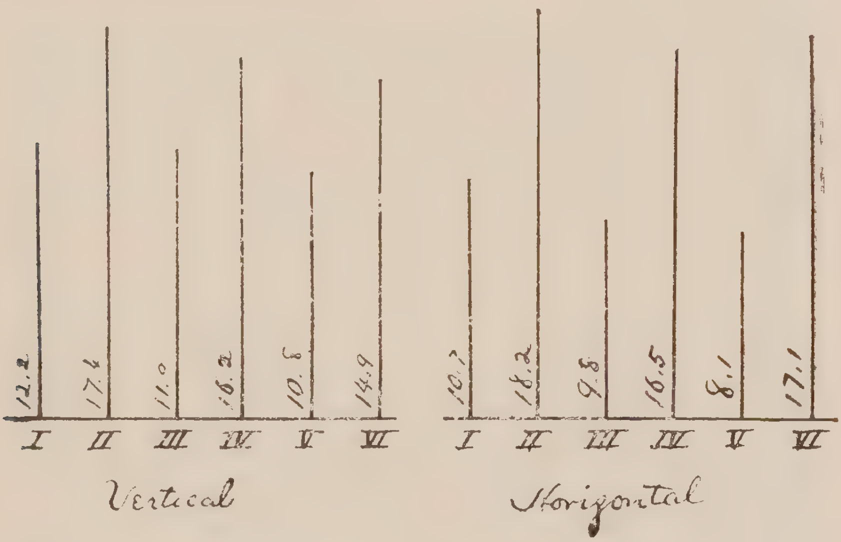
The following are the figures: —

Right hand.	Pronation, eyes open (I.) 22; eyes closed (II.) 5.		
	Supination, eyes open (III.) 29; eyes closed (IV.) 9.		
	Midway, eyes open (V.) 30; eyes closed (VI.) 10.		
Left hand.	(I.) 10.	(II.) 2.	
	(III.) 13.	(IV.) 2.	
	(V.) 15.	(VI.) 4.	
Being approximately R.	1-4	1-3	1-3
L.	1-5	1-6	1-3

Group II. consists of those tracings in which some decided movement of the lines in some particular direction from the starting point is evident. In these the hand has not been held more or less approximately in the same position, but has moved gradually in one direction or another. This is

entirely independent of its general steadiness or tremulousness (shown by the firmness of the line).

In order to obtain the amount of movement of the hand from the starting point in each direction, that is, the total excursion, the length and breadth of all the tracings were carefully measured. As before stated, the rod was so arranged that the tracing was in each case four times the size of the actual movement.



The average excursion in each position for all tracings (Groups I. and II.) is seen in the above diagram. The measurements were taken between the extremes in the vertical and horizontal planes. In series I. III. and V., that is, those with eyes open, the height of the lines diminishes in the order named in both planes and with either hand. The same holds good in series II., IV., and VI., where the eyes were closed, except that with the left hand VI. is six mm. longer than IV.; but if we add the averages of both dimensions VI. is less than IV. in all cases.

The average excursion in both planes in millimetres is as follows : —

	I.	II.	III.	IV.	V.	VI.
Eyes open,	9.8		9.09		7.6	
Eyes shut,		17.8		9.7		10.5

Now, if we compare the relative stability, as thus shown, with that obtained in our previous consideration of Group I., we find it also to be least in pronation. Thus the position of pronation gives the greatest excursion both when all the tracings are

considered, and likewise when only those of Group I. are measured.

No attempt has been made to draw any conclusions in regard to direction in these tracings, as the second series was much more valuable for that purpose.

As to the amount of tremor in the various positions, no difference was detected between pronation and supination. In the midway position less tremor is observed.

For these reasons the position of pronation was considered the most favorable for our purpose, and another point which influenced us in this selection is that this is the position into which the hand most naturally falls when the arm is extended. It is rare that any one, when extending the arm to place the finger in the thimble, holds it in any other position than that of pronation.

The second series of tracings, one hundred and eighty-five in number, were taken from men between the ages of twenty and thirty, who were undergoing a competitive physical examination and were hence presumably as healthy a set of individuals as could well be found. Nearly every form of ordinary occupation and trade was represented, the liberal professions, of course, excepted. The tests were made previous to the physical examination, when the men were fresh and in good condition. The position used was pronation, and each hand was tested separately and with eyes both open and closed. Each test lasted thirty seconds.

In the study of these tracings the same classification is used as in the previous series. They too are divided into the two groups of the stable and unstable (stationary and mobile).

In considering the direction of the movement, it is evident that the tracings of Group I. can afford us no information; they have therefore been placed in the table under the heading "stationary."

This group is of interest mainly in reference to the relation between the results obtained with the eyes open and with the eyes closed.

Of tracings with the right hand and with the eyes open it is seen that one hundred and forty-one (76.21 per cent.) are in this group, while of those with the same hand while the eyes are closed only twenty-six (14.05 per cent.) belong here. With the

left hand and the eyes open one hundred and sixty (86.44 per cent.) belong in this group, but of those taken with the eyes closed but thirty-six (19.45 per cent.).

Hence with the right hand there is a little more and with the left hand a little less than five times the control with the sight than without it. (This of course does not mean to imply that a given tracing with sight is five times more "stable" than a similar tracing when the eyes are closed, but only that five times as many "stationary" tracings are made when the hand is aided by vision as when not thus aided.)

In all the tracings of Group II. the movement assumes a definite direction, and in the table before us the direction of this movement is shown. The table is arranged so as to give the number and percentage of tracings in each direction in each of the four conditions. The conditions of the tracings are noted by the Roman numerals at the beginning of each line: I. right hand with eyes open; II. right hand, eyes closed; III. left hand, eyes open; IV. left hand, eyes closed. The remarks at the top of the columns show the direction. Thus, of tracings with the right hand and with the eyes open, in nine the hand moved to the right and in sixteen to the left, in nineteen downwards, in no case upwards. Of those which moved to the right,

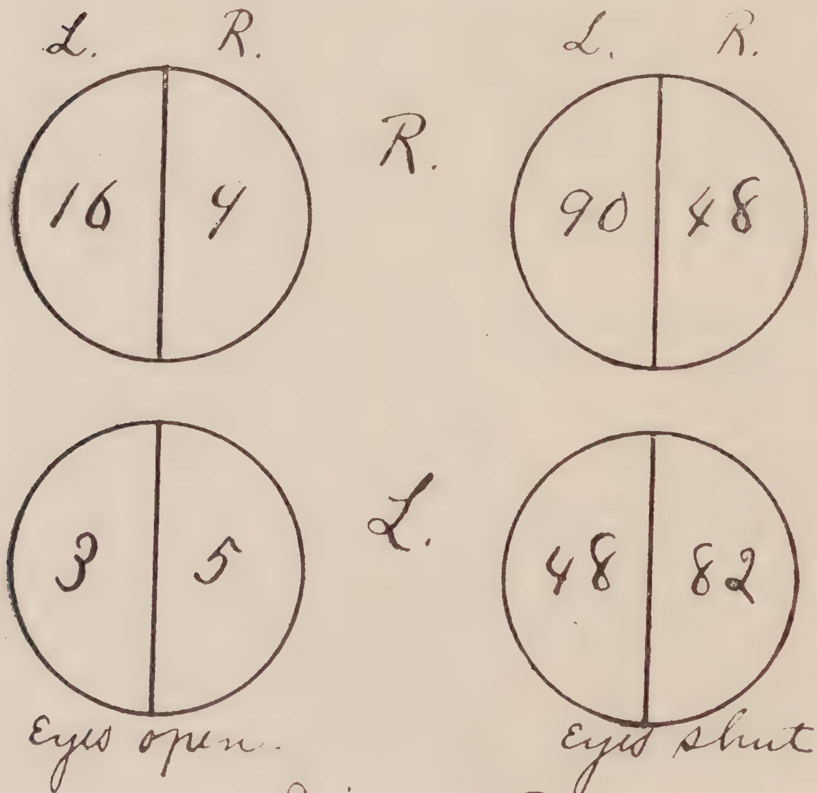
Position.	Stationary.	R.	L.	Down.	Up.	R. & Up.	L. & Up.	R. & Down.	L. & Down.	Horizontal & R.	Horizontal & L.	Total Down.	Total Up.
I.	141	9	16	19	0	0	3	6	8	3	5	33	3
	76.21	4.36	8.64	10.27	—	—	1.62	3.24	43	1.62	2.70	17.8	1.62
II.	26	48	90	19	2	10	7	26	66	·12	16	112	19
	14.05	25.94	48.64	10.26	1.07	54	3.77	14.05	35.67	6.43	8.64	60.54	10.27
III.	160	5	3	14	3	0	0	3	3	2	0	20	3
	86.44	2.70	1.62	7.56	1.62	—	—	1.62	1.62	1.07	—	10.81	1.62
IV.	36	82	48	16	3	7	12	55	25	20	11	96	22
	19.45	44.32	25.94	8.64	1.62	3.77	6.43	29.72	13.51	10.81	5.94	51.89	11.89

six moved downwards in addition and three horizontally only. The total in which the general direction of the movement of the hand was downward was thirty-three, upwards was three.

In a very few cases it may be seen that the hand rose, but so small is their number that they are probably due to accidental causes or to an over-estimated effort to prevent the hand from falling. The percentage in which the hand falls is large, especially when the eyes are closed. In the latter case, with the right hand, it is nearly four times as large as when the eyes were open, and with the left hand nearly five times as large.

When the lateral direction is considered, a more interesting result is observed, namely, that the tendency of the hand is to move toward the median line of the body, and this is so whether the eyes be open or shut.

The following figures show the number of cases in which the hand moved in each direction:—

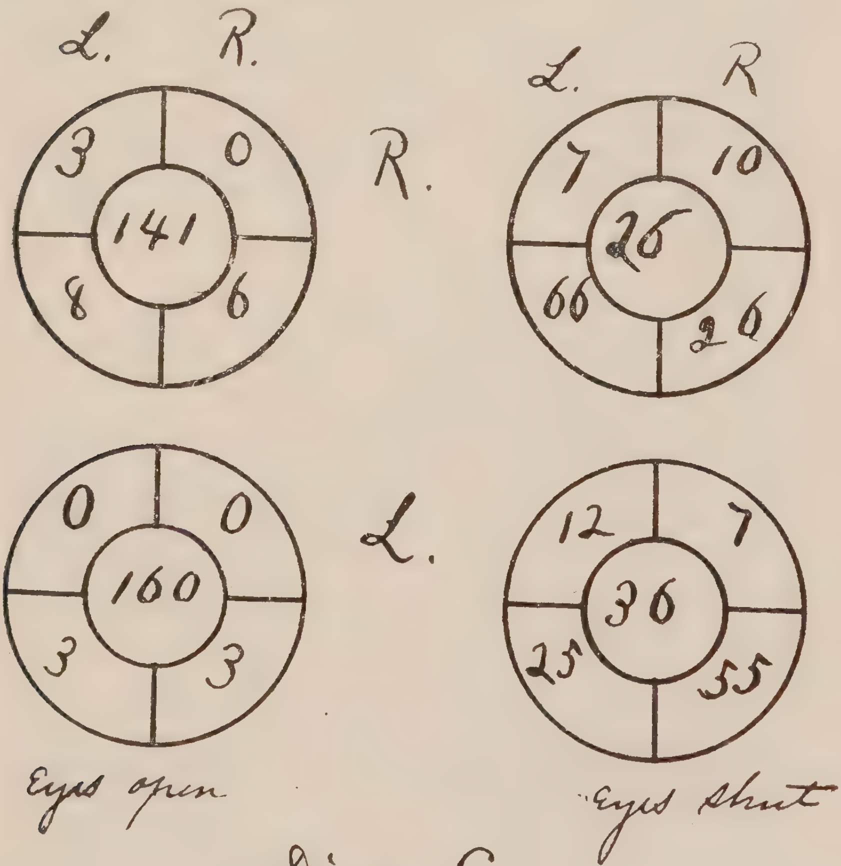


Diag. B.

These have reference to the lateral movement only. In a large proportion the direction was obliquely downward, but in a very few it was obliquely upward. With the eyes open only three moved obliquely upward, with the eyes closed thirty-six. When the eyes are closed and the hand is governed only by muscular sense, a large percentage fall toward the middle line of the body, in the proportion of a little more than two to one.

The number of tracings in which the hand moved obliquely upward when the eyes were closed is too small to enable us to form any definite conclusion in this regard, but in the larger number the tendency is to move in the same plane as the majority of those moving downwards, but in an opposite direction.

The figures, Diagram C, express these results diagrammatically, giving in the centre the number of stationary tracings, and in the circumference those moving in each oblique direction.



Diag. C

The amount of deviation from the starting point was also considered in order to determine the relative steadiness (stability) under the different conditions. For this purpose the greatest height and width of each tracing was measured.

These measurements were as follows. Averages of Group I. (stable tracings):—

I.	Right hand, eyes open.	Length 6.57 mm.	Breadth 5.76 mm.
II.	" eyes closed.	" 5.43 mm.	" 9.01 mm.
III.	Left hand, eyes open.	" 6.24 mm	" 5.27 mm.
IV.	" eyes closed.	" 7.93 mm.	" 7.43 mm.

Group II. The tracings vary considerably in size, as will be seen by comparison of the extremes:—

Position I.	Length.	Greatest 12.70 mm.	Least 2.00 mm.
	Breadth.	" 15.00 mm.	" 2.75 mm.
II.	Length.	" 20.00 mm.	" 3.75 mm.
	Breadth.	" 16.20 mm.	" 4.00 mm.
III.	Length.	" 12.00 mm.	" 2.50 mm.
	Breadth.	" 9.50 mm.	" 1.50 mm.
IV.	Length.	" 16.20 mm.	" 3.25 mm.
	Breadth.	" 15.70 mm.	" 1.50 mm.

If we compare now the average measurements of the tracings in both groups according as they were drawn with the eyes open or closed, we find a decided loss of control when the aid of sight is removed.

Averages, Groups I. and II.

Right hand, eyes open.	Length 7.70 mm.	Breadth 6.30 mm.
" eyes closed.	" 12.85 mm.	" 14.10 mm.
Left hand, eyes open.	" 8.42 mm.	" 5.26 mm.
" eyes closed.	" 11.42 mm.	" 12.99 mm.

RESUME.

In a very large proportion of cases with the eyes open the hand is kept approximately in the original position. With the eyes closed this number is comparatively small, being about one fifth of the preceding. The average excursion of the hand in these cases, which is a measurement of its steadiness (stability) is 6.16 mm. for the right and 5.75 mm. for the left with the eyes open, and 7.22 mm. for the right and 7.68 mm. for the left with the eyes closed, the steadiness of the left hand being in the former ~~each~~ case greater than that of the right. The tracings, however, vary very considerably, the extremes being 2 mm. and 20 mm. in the vertical, and 1.50 mm. and 16.20 mm. in the horizontal, planes.

Occupation or training exerts more or less influence on the power to hold the hands in a fixed position (steadily), clerks and those accustomed to use the hands for fine work showing more power

in this respect than men accustomed to rough physical work, as laborers, teamsters, etc. A number of records of professional men, not included in those under consideration here, show as a rule much more control than those of any other class examined. The power of control of muscular effort seems to keep pace with mental development. The method of various persons in attempting to control their hands during this test shows this strikingly. Persons accustomed only to rough and unskilled labor are, as a rule, unable to perform the more delicate motions, and are likewise less able to co-ordinate for unusual movements.

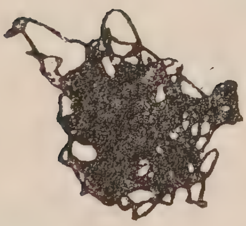
The number of tracings in which the hands moved upwards are too few to enable us to draw useful deductions. In the greater number the elevation was but slight, and may have been due to an over-estimated effort to prevent the hand from falling.

When, however, the hand descends a strong tendency to fall towards the median line is observed.

When the hand deviated considerably from the starting point with the eyes shut, the subject had as a rule no knowledge that it had done so, and was much surprised on opening his eyes to discover what had occurred.

As far as could be judged the functional disturbance arising from nervousness had no influence

PLATE I.



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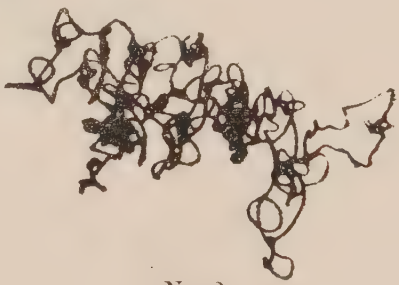

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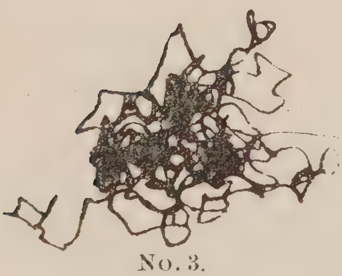


No. 3.

No. 4.

PLATE II.

Finger Tracings (natural size).

Figures I. 1, I. 2, I. 3, I. 4, show an ordinary series of finger tracings, all taken from the same person under one of the four conditions. I. 1. Right hand, eyes opened. 2. Right hand, eyes closed. 3. Left hand, eyes open. 4. Left hand, eyes closed. This is a typical series of the "regular" type. There is very little variation in the extent of the movement under the four conditions. The direction, however, varies: 1 is indeterminate, 2 upwards and to the left, 3 and 4 upwards and to the right.

Figures II. 1, 2, 3, 4, show another series of finger tracings taken in like manner (all from one person). They show well the contrast which often occurs when the eyes are open and when they are closed. 1 and 3 eyes open, 2 and 4 eyes closed. Control of vision apparent.

to increase the deviation from the starting point in any special direction, though it sometimes increased the size of the tracings of Group I. The disturbance from this cause, however, was much less than might at first sight be expected, the unsteadiness in great measure disappearing with the voluntary effort. In two cases, however, the disturbance from this cause was so great that all efforts to comply with the test were futile. With the attempt at muscular fixation the hand was thrown into so severe a spasmodic action that no control was possible. So far as could be determined in these cases no alcoholic influence and no organic disease existed. One of these subjects was a man who passed the physical examination for the police force, the other a woman debilitated by hard work and care.

The tremor can be estimated only by a close study of the tracings. It is totally unconnected with their "stability," or the amount or direction of their deviation. There is probably no tremor registered by this instrument which cannot easily be seen, and the study of its character is complicated by the presence of the coarse movements in the tracing.

Many of the fine tremors are not registered, because the slight support given by the thimble causes them to cease in the finger tested, although the other fingers continue their motion as before.

Hard-working laborers present no special tremor, but have less "stability" than those accustomed to more delicate work, but such tremor as existed in them was not overcome by mere contact with the instrument.

The emotional tremor is not a fine one. Its amount varies considerably with the individual, and frequently modifies or obscures any other tremor which may exist. It does not, however, seem to affect the control (stability) of the hand.

HEAD MOVEMENTS. STATIC EQUILIBRIUM.

The observations in regard to static equilibrium were made upon the same individuals as those from whom our second series of hand tracings was taken, that is, on healthy men who were undergoing a competitive physical examination, and whose ages were all, according to their declaration, between twenty and thirty, though it is

probable that a few may have been somewhat above the specified age. The total number examined was one hundred and eighty-one, and they were all tested in regard to their relative steadiness in the upright position, with the eyes open and with the eyes shut.

The method of examination was as follows. The men were made to stand with their heels together in military attitude, and with the eyes fixed on some object at a distance, yet easily seen. The apparatus used consisted of a square piece of board held in position on the head by plates of spring brass, and in appearance much like the cap of an Oxford student. On the upper surface of this board a paper was placed to receive the tracing. A horizontal rod was so arranged that it could be adjusted at the desired height, and to this a pen was secured by a hinge joint, which allowed a vertical motion only. In each case the time allowed was thirty seconds, and the pen was not allowed to touch the paper until a moment after the subject had taken the required position. The observation was first taken with the eyes open, and then, after an interval, repeated with the eyes closed.

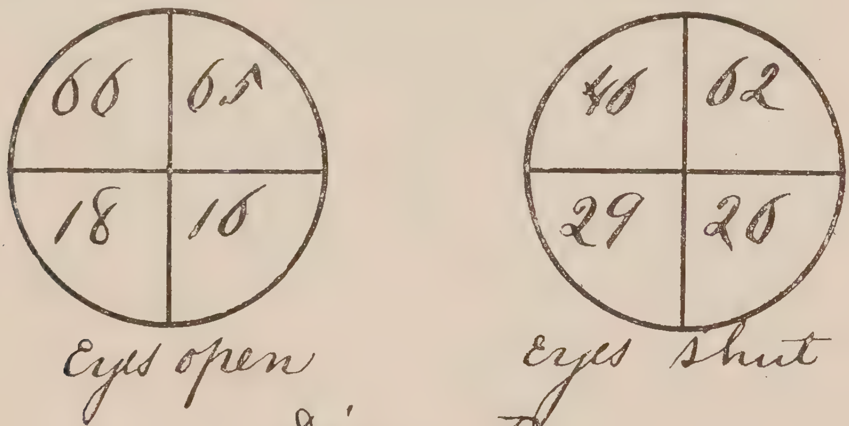
From these experiments we obtained the following results. No person stood absolutely still during the time of the trial, but some moved much less than others.

The direction of movement was as follows. With the eyes open one hundred and thirty-eight moved forward, and only thirty-five backward,— the remaining eight either moving laterally only, or the direction of movement being indeterminate. With the eyes shut one hundred and ten moved forward and fifty-eight backward, while in eleven the movement was either lateral only or indeterminate, or equally forward and backward. These figures show that the ordinary man, while standing unsupported, has a strong tendency to move the head forward slightly, and this seems to be more free to show itself when the eyes are open, and the person can perceive that he is not actually in danger of losing his balance, than with the eyes shut, when a stronger exertion may be made to keep upright.

In regard to lateral motion, we find that with the eyes open eighty-five moved to the right, and

eighty-six to the left, showing no preference for either direction. With the eyes shut, however, ninety-seven moved to the right, and seventy-eight to the left, showing a slight preponderance in favor of the former. Those not counted moved either nearly straight forward or backward, or the direction of their motion could not be determined.

In most of the cases however, as may be seen from the above, the motion was not simply an antero-posterior or a lateral one, but a combination of the two. Thus with the eyes open sixty-five moved forward and to the right, sixty-six forward and to the left, sixteen backward and to the right, eighteen backward and to the left. With the eyes shut, sixty-two moved forward and to the right, forty-six forward and to the left, twenty-six backward and to the right, twenty-nine backward and to the left (diagram). These figures show again the



Diag. D

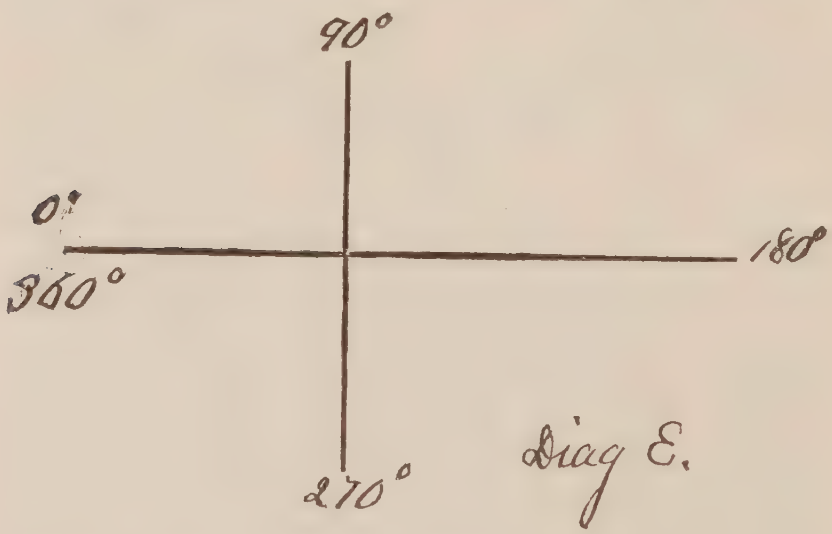
strong tendency to move forward in each case, rather than backward. They show also that this is less marked with the eyes shut than with the eyes open. With the eyes open, for those moving forward there is no preference shown for one side over the other, but with the eyes shut decidedly more move forward and to the right than forward and to the left. When moving backwards, a slight preference is shown, both with the eyes open and shut, for the left rather than the right. With the eyes shut there certainly appears to be a decided tendency to move either forward and to the right, or in the reverse direction, backward and to the left, rather than in the other diagonal,—ninety-one

(55.8 per cent.) moving in the one diagonal and only seventy-two (44.2 per cent.) in the other.

The foregoing figures show only the general directions of movement, but a number of measurements were also made to determine the position of the person at the end of the trial in relation to his position at the beginning. It was quite obvious, from an examination of the tracings, that the position at the end of the trial was not by any means always that furthest removed from the position at starting, but that, on the contrary, the subjects moved first in one direction, and then frequently backwards towards or even beyond the starting point,—these movements being, moreover, not always regular, but irregular, or more or less zigzag.

The directions of the points of ending from the starting points were measured on the tracings in degrees and minutes by means of a projector, and the following were the results obtained.

In only a certain proportion of cases could the two points be determined with sufficient accuracy to admit measurement of their relative direction. The direction was always measured from the starting point, which in all cases had been marked, to the point of ending. It was stated, as before



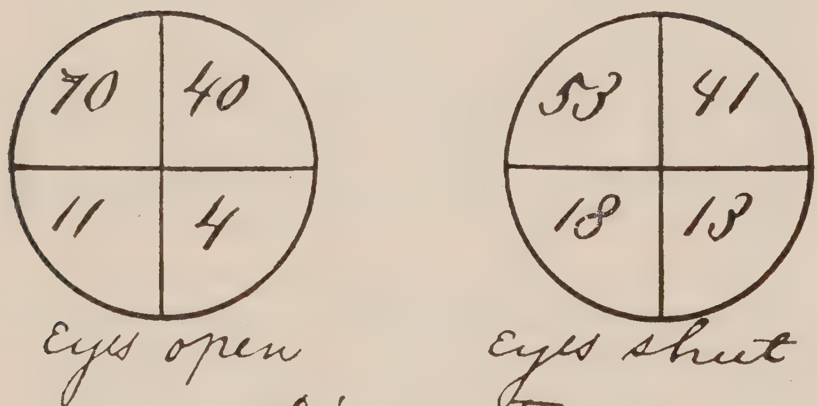
noted, in degrees and minutes,—the line of the board pressed on the paper being taken as the base line. Thus any line parallel to this ran from 0° to 180°, and its perpendicular from 90° to 270°.

Of one hundred and twenty-five cases with

the eyes open, in which these directions could be accurately determined, we found that in forty (32 per cent.) the ending point was upward and to the left of the starting point, since the position of the head at the end of the test was forward and to the right of that at the beginning. In seventy cases (56 per cent.) the position of the head at the end of the time was forward and to the left. In only eleven cases (8.8 per cent.) was it backward and to the left, and in only four cases (3.2 per cent.) backward and to the right.

From the same number of cases, with the eyes shut, we found that in forty-one (32.8 per cent.) the position of the head at the end of the time was forward and to the right of its position in the beginning. In fifty-three cases (42.4 per cent.) it was forward and to the left of its original position, while in eighteen cases (14.4 per cent.) it was backward and to the left, and in thirteen cases (10.4 per cent.) it was backward and to the right.

Position of Head.



Diag. F.

From these figures we see that at the end of a certain period (thirty seconds) in the normal man, standing upright and striving consciously or unconsciously to maintain a stable equilibrium, the tendency is that the head should be in front of its original position, and rather to the left than to the right. Moreover, we see here, as in our previous results, the greater backward tendency with the

eyes shut than with the eyes open, 24.8 per cent. of the former to 12 per cent. of the latter, or more than twice as many of those with their eyes shut, having had their heads back of the original position at the end of the half-minute, than of those with their eyes open.

The second question considered in these observations was the amount of movement. As the actual length of the lines could not readily be measured, this was determined by measuring the space covered by the lines. Thus the distance from the lowest point directly upwards to a point situated on the same horizontal line as the highest point, was considered as the absolute length. In other words, the distance between two straight parallel horizontal lines, the one passing through the lowest, and the other through the highest point, was considered the length; and similarly the distance between two straight parallel vertical lines drawn through the extreme points on each side was considered the breadth. All measurements were estimated in centimetres.

The measurements were calculated from one hundred and fifty cases as they came in direct order.

The average length with the eyes open was 3.764 cm., the greatest length in any one case 9.2 cm., the least 0.8 cm.

The average length with the eyes closed was 3.475 cm., the greatest length 8.9 cm., the least 1.2 cm.

The average breadth with the eyes open was 1.951 cm., the greatest in any case was 5.4 cm., the least 0.6 cm. (three cases).

The average breadth with the eyes closed was 1.963 cm., the greatest 6.8 cm., the least 0.4 cm.

From these results we can draw several conclusions. In the first place it is evident, as indeed is very apparent from the tracings, that the antero-posterior movement is, both with the eyes open and shut, much more extensive as a rule than the lateral movement. Secondly, that the antero-posterior movement is on the average decidedly greater with the eyes open than with the eyes shut, while the lateral movement is the same under both conditions, being if anything more marked when the eyes are closed.

In order to show the amount of variation, a calculation was made of the number of cases which were within the limits of each centimetre.

In the following table, in the column under zero, is found the number of cases which measured less than one centimetre; in the column headed one, those measuring between one and two centimetres, and so on.

Table showing amount of variation in antero-posterior movement with the eyes open.

0	1	2	3	4	5	6	7	8	9
1	20	37	48	25	11	9	6	2	1

This table reads: in one case only the length measured less than one centimetre; in twenty cases between one and two centimetres; in thirty-seven between two and three centimetres, and so on.

Measurements were also made to determine the distance between the starting point and the point of ending in each case, which shows the amount of variation in the position of the head at the moment of ending from that at the beginning. The average in one hundred and twenty-five cases with the eyes open was 2.840 centimetres; the greatest distance was 7.7 centimetres; the least was 0.2 (two cases). In the same number of cases with the eyes closed the average was 2.716 centimetres, the greatest distance was 8.0 centimetres, the least was 0.3 centimetre (two cases).

CONCLUSIONS.

I. The ordinary healthy man, when standing erect unsupported, has a tendency to move forward slightly, and this tendency seems to be more marked when the eyes are open.

II. Those in whom there is a forward movement with the eyes open show no preference in regard to the direction of the lateral movement combined with it, the number moving forward and to the right, and the number moving forward and to the left being nearly equal. When the eyes are closed, however, there is a stronger tendency to move forward and to the right than forward and to the left. Moreover, more persons with the eyes closed moved backward and to the left than backward and to the right.

III. The position of the head at the end of half a minute was found in a large majority of cases to be in front of the position at starting, and a larger number were forward and to the left than forward and to the right. This applies both when the eyes were open and when they were closed.

IV. More than twice as many persons, when their eyes were closed, had their heads back of the original position as did with their eyes open.

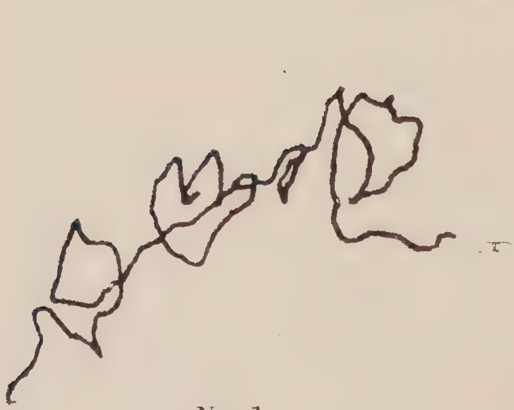
V. The extent of the antero-posterior movement was somewhat greater with the eyes open than with the eyes closed. The extent of the lateral movement was practically the same under the two conditions.

VI. The average extent of the antero-posterior movement was much greater than that of the lateral movement. In the majority of persons there was but little individual variation in the extent of the movements.

VII. The average distance of the head at the end of the time from its position in the beginning was about 2.8 centimetres with the eyes open, and 2.7 centimetres with the eyes closed.

We cannot conclude this paper without drawing attention to the very interesting article of Dr. Hinsdale on this subject in *The American Journal of Medical Sciences for* 1878, where the first reported observations on these head movements are detailed.

PLATE III.



No. 1.

No. 2.

PLATE IV.

No. 1. No. 2.

PLATE V.

No. 1. No. 2.

Head Tracings (natural size).

In each case the tracings are taken from a single individual with the eyes open (2), and with the eyes closed (1). In all cases they are normal tracings.

III. These are typical tracings, rather mobile. The direction of movement is rather uncommon, being in both backward and to the left.

IV. Typical tracings, rather stable; direction of motion forward and to the left.

V. Typical tracings, average movement; direction forward and to the right.

We desire to express our special thanks to our friend, Dr. McCollom, to whose kindness we are indebted for the opportunity of making many of our observations, and also to Dr. Rufus A. Kingman, to whose skill we owe the excellent photograph of the tremograph.

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AN EXPERIMENTAL INVESTIGATION OF STRYCHNINE POISONING. By ROBERT W. LOVETT, M.D., *Boston.*

From the Physiological Laboratory of the Harvard Medical School.

THE most superficial consideration of the action of drugs upon the higher organism shows a constant and marked affinity on the part of certain drugs for certain organs, or to speak more correctly, a power in certain organs to select some substance from the circulation, and by destroying or excreting it or by storing it up, to remove it from the blood. Urea circulates freely through the body as a constituent of the blood and no organ or gland shows any special disposition to select it from the blood current until the kidney is reached when it is immediately removed and disposed of. The liver shows a marked aptitude for selecting and filtering out from the blood certain products of digestion which in the general circulation prove harmful or even poisonous. Peptones, as shown by Schmidt and Mulheim[1], in the general circulation are distinctly poisonous, but when the blood containing them passes through the liver they are removed and broken up into useful glycogen and sugar[2]. In the same way nicotine, conium, etc. are much less poisonous to the organism when they have passed through the liver[3], and that this is an elective affinity and not a mere removal of all poisons by the liver is shown by its inability to remove or render less harmful such drugs as curara, atropine, or Prussic acid[4].

The well-known effect of curara upon the motor nerves, veratrine and physostigmine in their distinct effect upon the muscles, the storage of lead in the nervous system, and many other examples all emphasize this elective affinity of drugs, yet there is a large class of cases in which it is impossible to say whether organs which are particularly affected by certain drugs have a special attraction for

[1] Ludwig's Work.

[2] Seegen. *Pflüger's Archiv*, XXVIII. 990.

[3] Lautenbach. *Phila. Med. Times*, 1877, May 26.

[4] Brunton. *Mat. Medica, Pharm. and Therapeutics*, Am. Ed., p. 349.

these drugs and select them from the circulation to appropriate them and store them up in their own structure or whether the organ in question is merely particularly susceptible to the action of these drugs. For example, does strychnine exercise its peculiar power upon the spinal cord because there is more of the drug present there than in the other organs or because the cord is more susceptible to its action than the muscle, the brain or the liver for instance?

In the hope of obtaining an answer to at least part of this question and of getting a more extended view of the principle of this elective affinity the following work was undertaken at the suggestion of Prof. Henry P. Bowditch and carried on under his direction in the Physiological Laboratory of the Harvard Medical School. The line of investigation was, to ascertain if the spinal cord showed any especial facility in taking from the circulation and storing up in its tissue such substances as strychnine for instance, a drug whose presence in the organism is demonstrated by symptoms of spinal origin. This line of inquiry was particularly brought to notice by the method of Pasteur in selecting the spinal cord of rabbits as the source of virus for further inoculation in the prevention of rabies.

Sulphate of strychnine is particularly well adapted for use in such experiments, as it affects animals in a uniform and definite way and inasmuch as the quickness of its effect is proportionate to the size of the dose it offered the hope of establishing a quantitative table. It is a drug which acts particularly upon the spinal cord. Müller and Magendie attributed its action to some structural or electrical change, but that idea has of course passed away and now it is regarded not so much as an excitor of the spinal cord as that it renders the cord more excitable to reflex stimuli. As Brunton puts it, "Its action is probably caused by alteration in the comparative rate of transmission of stimuli from one cell to another[1]." Frenchmen speak of it as "exaltation excessive de l'excitabilité réflexe des centres bulbo-médullaires," and such a theory has for its supporters Brown-Séquard, Vulpian, Van Deen, Mayer, etc. Magendie showed very clearly that the poison acts through the circulation and that it produces its effects through the spinal cord[2]. Its elimination is prompt and is effected by means of the urine and the saliva. Kratter[3] found it present in the urine of a patient half an hour after its subcutaneous

[1] Brunton. *Loc. cit.*, p. 150.
[2] Magendie. *Paris Soc. de Philom. N. Bull.* 1805, I. 368.
[3] Kratter. *Wien. med. Wochenschrift* 1882, 8 to 10, p. 214.

injection and in 48 hours it had wholly disappeared. An experiment of Arnold bears on the same subject; he injected into a frog ·000001 grams of strychnine and noticed only a slight intoxication in the way of increased excitability in half an hour. But frogs who had had tetanic convulsions on the previous day for an hour after the administration of ·0001 of strychnine and had recovered, had tetanus and died after the administration on the succeeding day of ·000001 grams.[1] Frogs were chosen as the animals to be used as they are fairly susceptible to the effects of strychnine. I give the table of Nothnagel[2] showing the susceptibility of different animals to the drug:

	Minimum fatal dose for each kilogram of weight.		
Man	0·4	mgr.	(Husemann)
Rabbit	0·6	,,	(Falck)
Cat	0·75	,,	,,
Dog	0·75	,,	,,
Cock	2·00	,,	,,
Frog	2·1	,,	,,

The line of work was to be as follows: first by the injection of known amounts of strychnine into frogs of known weight and by the observation of the time in which convulsions occurred, to construct a table showing the time that various amounts of strychnine require to produce convulsions in frogs of known weight. And second, to ascertain if any difference exists in the relative amounts of the poison contained in the tissue of the various organs at a definite time after either the introduction of the drug into the system or the occurrence of convulsions. And more definitely to determine if possible by means of the table mentioned above the amount of poison present in an equal weight of spinal cord, muscle, liver, blood, etc. of the poisoned frogs.

First then as to the formation of a quantitative table. The frogs used were for the most part small ones, as they of course reacted more quickly to strychnine than medium sized or large frogs. They were of the green and spotted varieties, for the most part freshly caught and kept for use in a tank of running water in a cool cellar, and only active and apparently healthy frogs were selected. The solution of strychnine sulphate used in the first set of experiments contained ·0648 mgr. (1/1000 of a grain) of the drug to 1 c.cm. of distilled water. An amount of this solution corresponding to the desired amount of the drug was

[1] Arnold. *Hygen.* XVI. 1.

[2] Nothnagel and Rossbach. *Nouv. et de Mat. Méd. et de Thér. Paris*, 1880.

injected into the dorsal lymph sac by means of a small and very sharp hypodermic needle; the forefinger was held for an instant over the opening after the syringe was withdrawn and the frog placed under a bell-jar to await the coming on of the convulsions. The first spontaneous tetanic convulsion was selected as the indication of complete strychnine poisoning, as it seemed the one definite phenomenon in the series of symptoms to fix upon.

After injection the frog would hop about smartly for a minute under his glass and then settle down quietly for a variable time. Then came a period of increased sensory and motor activity during which he sat up very straight, held his head up very high and looked interestedly about him. To this quickly succeeded a time of restlessness, and then he began to hop stiffly about, showing a tendency to tetanus when he tried to draw up his legs after each hop. At this time, and before it, rattling the glass or touching the frog would cause a sharp sudden jump of this character. Finally in this condition he would give a sudden jump, often with a croak, and straighten out perfectly rigid and remain so. This was the sign adopted.

By the injection of various amounts of the drug into frogs which were afterward weighed Table I. was constructed. For instance, in Experiment 1, a frog weighing 18 grams received 1 c.cm. of the solution, which contained ·0648 mgr. of the drug, and in 11 minutes he had spontaneous tetanus, that is for each gram of his weight he had convulsions in ·87 minutes. It will be noticed that the time to convulsions for each gram of frog weight in any one dose vary a good deal more than one would expect, but the experiments stretched over several months and winter and summer frogs were used and the difference in the vitality of the two kinds is well known. There was also a marked variation in the individual susceptibility of the frogs which was unaccountable. At first it seemed that green frogs reacted more quickly to the drug than spotted frogs, but an analysis of the two classes showed that there was no practical difference.

Having then in Table I. established the time in which a frog weighing one gram would have had convulsions after the administration of each of the five doses of the drug varying from ·0648—·013 mgr. (1/1000—1/5000 of a grain) it was desired to represent the result graphically, which can be done by taking the abscissa to represent units of weight of the drug administered (hundredths of a milligram) and the ordinate to show the average time in minutes between administration and convulsions in a one gram frog. A working curve

TABLE I.*

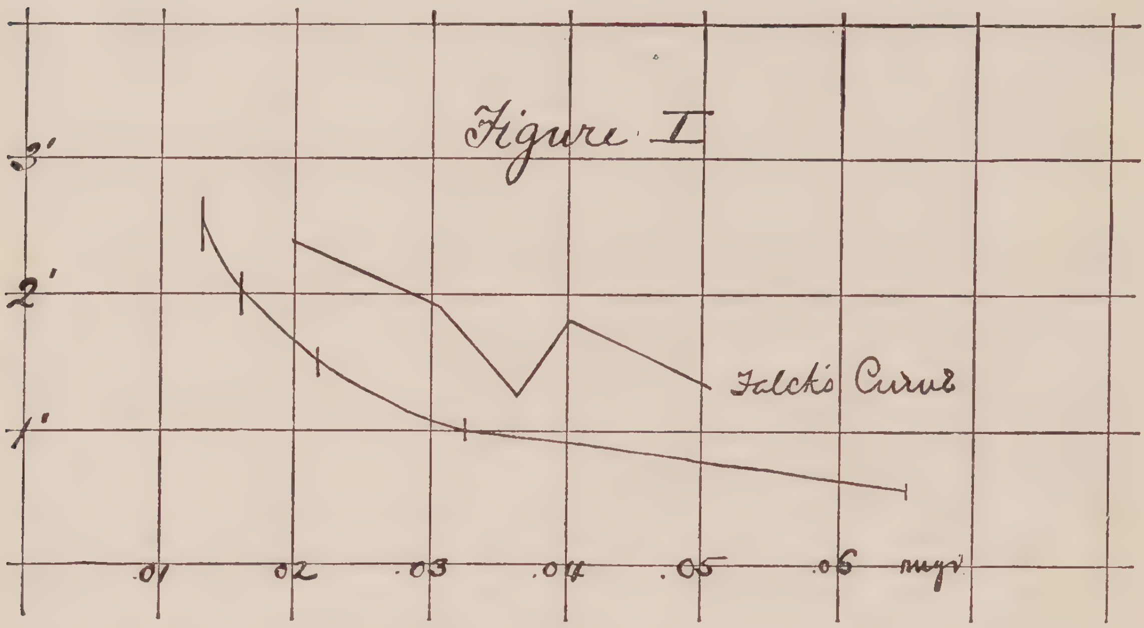
Dose ·0648 mgr. $\frac{1}{1000}$ grain.				Dose ·0324 mgr. $\frac{1}{2000}$ grain.				Dose ·0216 mgr. $\frac{1}{3000}$ grain.				Dose ·0162 mgr. $\frac{1}{4000}$ grain.				Dose ·013 mgr. $\frac{1}{5000}$ grain.			
No. of Expt.	Weight of frog.	Time to conv.	Time to conv. per grm. of frog wt.	No. of Expt.	Weight of frog.	Time to conv.	Time to conv. per grm. of frog wt.	No. of Expt.	Weight of frog.	Time to conv.	Time to conv. per grm. of frog wt.	No. of Expt.	Weight of frog.	Time to conv.	Time to conv. per grm. of frog wt.	No. of Expt.	Weight of frog.	Time to conv.	Time to conv. per grm. of frog wt.
	grms.	mins.	mins.		grms.	mins.	mins.		grms.	mins.	mins.		grms.	mins.	mins.		grms.	mins.	mins.
1	13	11	·87	14	12·5	6	·48	31	9	14	1·5	47	10	30	3·0	55	9	20	2·22
2	15	7	·46	15	12·5	7	·56	32	10	10	1·0	48	10	24	2·4	56	12	36	3·0
3	17·5	9	·51	16	12	10	·83	33	12·5	19	1·5	49	12	15	1·25	57	12·5	36	2·88
4	17	10	·58	17	13	10	·77	34	12·5	20	1·6	50	12·5	13	1·04	58	12·5	14	1·12
5	17	5	·29	18	17	10	·59	35	15	11	·73	51	12·5	25	2·0	59	14	45	3·21
6	20	16	·80	19	17·5	13	·73	36	16	28	1·73	52	20	37	1·8	60	17·5	34	1·94
7	21	18	·85	20	17·5	13	·73	37	17·5	10	·57	53	21	48	2·28	61	17·5	65	3·71
8	22	25	1·10	21	18	33	1·83	38	17·5	14	·80	54	23	60	2·61	62	17·5	46	2·63
9	22	10	·45	22	19	18	·95	39	18	19	1·05					63	18	34	1·80
10	23	23	1·0	23	19	20	1·05	40	18	31	1·72								
11	23	18	·78	24	19	31	1·63	41	19	40	2·10								
12	27	12	·44	25	19	39	2·05	42	20	21	1·05								
13	29	10	·34	26	20	31	1·55	43	21	60	2·88								
				27	23	17	·74	44	22	45	2·04								
				28	23	20	·87	45	22·5	50	2·22								
				29	28	15	·53	46	22·5	34	1·51								
				30	35	42	1·20												
Average of 13 Expts. ·657 minutes for each gram of frog's weight. Probable error ± 0·05.				Average of 17 Expts. 1·0. Probable error ± 0·08.				Average of 16 Expts. 1·5. Probable error ± 0·10.				Average of 8 Expts. 2·05. Probable error ± 0·16.				Average of 9 Expts. 2·54. Probable error ± 0·21.			

* Additional Experiments not included in table.

Amounts used.	Weight of frog.	Time to conv.	Amounts used.	Weight of frog.	Time to conv.
·648 mgr.	14	2	·162 mgr.	17·5	3
($\frac{1}{100}$ grain).	17·5	$2\frac{1}{2}$	($\frac{1}{400}$ grain).		
·324 mgr.	17·5	2 and 3	·081 mgr.	17·5	4
($\frac{1}{200}$ grain)	15	2	($\frac{1}{800}$ grain)	17	4

In dealing with larger quantities than 1/1000 of a grain it was found that after such a dose frogs of medium weight died in from two to four minutes without much regard to the amount of the poison used, 1/600 of a grain killing in the same time practically that 1/100 did. The same fact will be noted in the table from Falck given below where it will be seen that doses varying between 5 mgr. and 0·5 mgr. killed the frogs in practically the same times. In using smaller amounts than 1/5000 of a grain the results in time were so variable and so long delayed that they seemed of little value in establishing a quantitative table, as can be easily seen from the character of the curve in Fig. 1.

made in this way is seen in Fig. 1 and for purposes of comparison the experiments of Falck falling within the same dose limit are represented



in the same way although the latter curve is for the most part constructed from only one experiment for each dose. The correspondence in a general way of the two curves can be seen, with the difference however that Falck's times are all longer and the whole curve consequently above mine. The probable error[1] for each of the five doses has been calculated by Dr. J. W. Warren and its amount is represented by the length of the cross lines at the five points of observation. It is interesting to note how rapidly it increases with the diminution of the dose. The range of variation of the observations increases in the same way even more rapidly as the dose diminishes, but it hardly seems worth while to introduce it in the figure.

The work of Falck[2] was rather directed to determining the smallest fatal dose of strychnine in different classes of animals, but he publishes a quantitative table for frogs which deals with rather larger amounts.

It is given here for purposes of comparison. The method used was injection into the dorsal lymph-sac.

[1] *Boston Medical and Surgical Journal*, cxi. 8, 169.

[2] Falck, *Vierteljsch. f. ger. med.*, 1874, xx. 2. Cf. Pickford, *Arch. f. Phys. Heilkunde* 3 Jahrg. W. Arnold, *Hygen*, xvi. 1 (who did work in the same line without however considering the question of relative dose at all).

Dose of strychnine injected into dorsal lymph sac. (mgr.)	Weight of frog. (grms.)	Time to tetanus.	Time for each gram of weight.	
5	22	3·5′		
5	22·5	3′		
5	38	2′		
5	62·2	2·5′		
1	22	3·5′		
1	27·5	2·5′		
1	29·5	4·5′		
1	39	2·5′		
·5	24	5·5′		
·5	25	4′		
·5	29	5′		
·5	36	4′		
·1	26	15·5′		
·1	27·5	16′		
·1	31	21·5′		
·05	23	31′	1·3′ average 1·38′	These experiments are the ones represented in Fig. 1.
·05	25	26′	1·0′	
·05	34·5	79′	2·3′	
·05	53	49′	·93′	
·04	17	29′	1·8′	
·036	17	23′	1·3′	
·03	16	31′	1·9′	
·02	13	31′	2·4′	
·01	12	—	no convulsions	
·005	2	31′		
and no smaller amount gave convulsions.				

From his experiments Falck concluded that for each kilogramme of frog weight

(1) less than 0·5 mgr. of strychnine gives no convulsions,

(2) less than 2 mgr. of strychnine is not fatal,

(3) that 1 mgr. or more will give tetanus.

Having then established a quantitative table by which to measure, within the above mentioned limits, the amount of strychnine administered, the second part of the investigation was taken up, namely, the relative amounts of strychnine absorbed by the cord, the blood, the liver, the muscle &c. A frog of medium or large size was chosen and a solution of strychnine sulphate containing from 6·48 mgr. to ·0648 mgr. was injected into the dorsal lymph sac, and either immediately after tetanus set in or a stated time after the injection the dorsal lymph sac was opened and the entire skin of the back was removed so that the poison might be thoroughly washed away before removing the cord. The base of the skull was then separated from the vertebral column and the cord removed in the usual way by cutting away the arches of the vertebrae with a fine pair of scissors. The abdomen was then opened and a small piece of the liver removed, and the muscle was taken from the thigh; all this was done as quickly as possible. If blood was used it was collected first by plunging scissors into the thorax and cutting the front of it open and the heart would then appear in the opening from which the blood was dropped into a watch-glass. The cord was next carefully weighed, and the same amount of liver, muscle and blood weighed out separately. Each of these was then rubbed up in a porcelain mortar until it was very finely subdivided, a cubic centimeter of water, more or less, added, the pestle was washed off and the whole solution of each organ was drawn up into a hypodermic syringe with a fairly coarse needle. Each solution was injected into the dorsal lymph sac of a small frog. The method of injection was the same as before except that to avoid leakage from the larger opening necessarily made by the needle, it was first plunged into the muscular substance of the upper thigh, carried a short distance through it and then brought into the dorsal lymph sac, the muscle at once closed down on the withdrawal of the needle and closed the opening. Each frog was placed at once under a bell-glass, and the time noted when spontaneous convulsions came on, he was then weighed and from Fig. 1 the amount of strychnine which he had received was easily obtained when it fell within the limits. Certain cases however fell outside the limits of my curve. It will be seen easily that when it took longer than 2·54

minutes to convulsions for each gram of frog weight I had no means of judging the amount of strychnine present except to say that it was less than my minimum quantity. I could however often compare the relative amount received by two frogs when the quantity was far too small to be measured by the table, by the amount of intoxication at a definite time after injection or by the occurrence of long delayed convulsions.

TABLE II.

The dose in each case was 6·48 mgr. of strychnine subcutaneously.

Cases where no tetanic convulsions occurred are marked with a star (*). In all other cases convulsions occurred.

From experiments on SECOND FROGS was determined the

WEIGHT OF STRYCHNINE IN EACH GRAM OF

No. of Exp.	Time to conv.	Wt. of frog. (grms.)	Wt. of each organ used. (grms.)	Time to removal of organs.	Spinal cord. (mgr.)	Liver. (mgr.)	Muscle. (mgr.)	Other organs. (mgr.)
1	2′	60	0·04	13′	1·52	0·325	0·377	ovary. no intox.*
2	8′	100	0·07	30′	0·19	< 0·19	< 0·19	brain. < 0·19
3	2′	46	0·05	20′	1·2 +	{0·39 {0·60	{< 0·26 {< 0·26	
4	3′	110	0·065	15′	1· +	< 0·2*	< 0·2*	brain. < 0·2*
5	3′	55	0·06	15′	< 0·25*	< 0·25*	< 0·25*	
6	4′	100	0·09	30′	0·164	< 0·14*	< 0·14*	brain. < 0·14*
8	2′	41	0·055	20′	< 0·23	< 0·23*	< 0·23*	
9	5′	45	0·07	5′	0·186	< 0·19*	< 0·19*	blood. 0·463

These experiments are given in Table II. To consider one experiment in detail: in experiment No. 1, a frog weighing 60 grams was given an injection containing 6·48 mgr. of strychnine sulphate into the lymph sac. In 2 minutes he had spontaneous tetanus and in 13 minutes from injection the organs were removed, ·04 grams of each were taken and triturated as described above. The solution of spinal cord was then injected into a frog weighing 12·5 grams and in 9 minutes he had convulsions, that is, for each gram of his weight he had convulsions in 0·7 minutes; a reference to Fig. 1 shows that this corresponds to an amount of the drug equal to ·0608 mgr.; estimating the amount of

the drug present in each gram of cord, to bring it into a uniform scale, we find one fourth of ·0608 present in each centigram which is ·0152, or 1·52 in each gram of cord. This last is the only figure given in the table in each case, as the introduction of all the figures would make it hopelessly complicated. In the same experiment a frog weighing 15 grams received the solution of liver and had convulsions in 40 minutes, showing 0·325 mgr. of strychnine present in each gram of liver. The same process showed ·377 mgr. present in each gram of muscle. Experiment 7 was thrown out on account of evident inaccuracy, as the muscle frog had evidently had strychnine not long before, although every precaution was taken to prevent using the same frogs twice. Experiment 5 was negative for some unknown reason, but with these two exceptions the series shows very plainly that in a given time after the subcutaneous injection of strychnine and the occurrence of convulsions the spinal cord of the poisoned frog contains for each unit of weight much more strychnine than the same amount of liver, muscle, brain, or ovary contains. Table III. shows a series of experiments where the drug was administered by the stomach. A hypodermic syringe armed

TABLE III.

The dose in each case was 6·48 mgr. given by the stomach except in experiment 13, where it was twice that amount (12·48).

Cases where no tetanic convulsions occurred are marked with a star (*). In all other cases convulsions occurred.

From experiments on SECOND FROGS was determined the WEIGHT OF STRYCHNINE IN EACH GRAM OF

No. of Exp.	Time to conv.	Wt. of frog. (grms.)	Wt. of each organ used. (grms.)	Time to removal of organs.	Spinal cord. (mgr.)	Liver. (mgr.)	Muscle. (mgr.)	Other organs. (mgr.)
10	3′	39	0·055	25′	< 0·23	< 0·23*	< 0·23*	
								blood.
11	5′	65	0·08	20′	< 0·16	< 0·16*	< 0·16*	< 0·16
12	4′	37	0·07	20′	0·308	< 0·18	< 0·18	0·728
13	3′	57	0·065	30′	< 0·2	< 0·2*	< 0·2	< 0·2

with a blunt coarse needle was thrust down the œsophagus for an inch and the solution slowly injected. On withdrawing the needle the frog would swallow vigorously and take down what might remain in his throat. The experiments showed the same point as those in Table III.,

except that when administered by the stomach the poison worked more slowly. The cord then absorbs more strychnine relatively than the liver or the muscle, whether the drug enters by the digestive or by the lymphatic system.

The next point to be determined was the relative amount of strychnine contained in equal weights of the blood and the cord. Doses of three sizes were given, 6·48 mgr. (1/10 grain), 0·648 mgr. (1/100 grain), and 0·0648 mgr (1/1000 grain) to see if the same relation would hold true for large and for small doses. Theoretically it seemed as if when a small amount was administered the cord would be obliged to absorb nearly all of it in order to produce convulsions and therefore

TABLE IV.

Showing the relative amounts of strychnine in the blood and cord.

Cases where no convulsions occurred are marked with a star (*). In all other cases convulsions were noted.

No. of Exp.	Dose in mgr.	Wt. of frog. (gr.)	Time to conv. & removal of organs.	Amount of each used. (grms.)	Weight of Strychnine in each gram of Cord. (mgr.)	Blood. (mgr.)	
14	6·48	36	4′	0·05	0·372	0·266	
15	6·48	35	3′	0·035	< 0·4	1·4	
9	6·48	45	5′	0·07	0·26	0·46	
16	6·48	40	3′	0·04	< 0·3	0·53	
17	6·48	38	5′	0·05	< 0·26*	< 0·26*	
18	0·648	31	7′	0·055	< 0·23*	< 0·23	
19	0·648	27	7′	0·05	< 0·23*	< 0·23*	But the blood frog here showed much greater intoxication than the cord frog.
20	0·648	33	4′	0·06	< 0·25*	< 0·25	
21	0·648	25	4′	0·055	< 0·23*	< 0·23	
22	0·0648	45	30′	0·055	< 0·23	< 0·23	The convulsions in the cord frog came on more quickly.
23	0·0648	45	15′	0·06	0·45	< 0·25	
24	0·0648	34	35′	0·09	< 0·14*	< 0·14*	The intoxication in the cord frog came on more quickly.
25	0·0648	50	13′	0·06	< 0·25	< 0·25*	
26	0·0648	26	23′	0·05	< 0·26	< 0·26*	

Note. The reason that the minimum weight of strychnine in the blood and cord varies so much is of course because different weights of the organs were taken for use in the first place.

would be found to contain relatively more than the blood, whereas in large doses the cord would be saturated to the point of convulsion and the blood would still contain a large amount and probably relatively more than the cord, and Table IV. shows practically how constant that relation is. 6·48 and 0·648 mgr. are to be accounted large doses and 0·0648 mgr. a small dose, and it must be remembered that in using so small a dose as 0·0648 mgr. in the first frog the secondary frogs often had no convulsions at all and the relative amounts of strychnine present had to be determined in many cases by the degree of intoxication present at the same time after injection. To make the Table simpler the weight of the blood and cord frogs and the times to convulsions in these frogs are omitted. With two exceptions, Experiment 14, and experiment 17 which was entirely negative, the series shows very clearly that at the time of the occurrence of convulsions with larger doses the blood contains much more strychnine than the cord relatively and that in small doses the reverse holds true. Experiments 11, 12, and 13 in Table III. show that the same holds true with regard to large doses administered by the stomach.

It may be noted here that doses of strychnine large enough to cause convulsions in frogs in one or two minutes kill them almost immediately often; and that after a more or less marked convulsion, sometimes after scarcely any convulsion at all, they lie limp and lifeless, a very different condition from the tetanic rigidity which follows smaller doses and persists for so long a time.

Without then attaching too absolute a value to the figures given above, it seems justifiable to state the conclusions as follows.

First, the spinal cord has the power of selecting strychnine from the circulation and of storing it up in its structure, for at the occurrence of convulsions or a definite time after the ingestion of a certain amount of the drug the cord is found to contain relatively more than the liver, the muscle, the brain, or the ovary. A moment's consideration will show that this is not due to the relative vascularity of these organs, for the liver contains relatively much more blood than any of the others of course. And secondly that at the time of convulsions the relative amount of strychnine in the blood and in the spinal cord vary altogether with the amount of the drug given to the frog. If it is a large dose, killing in 3·5 minutes, the blood contains overwhelmingly more, but if it is a small dose, causing convulsions in 15—30 minutes, a larger amount relatively is found in the cord than in the blood. It seems as if the amount of the drug necessarily present in the cord to produce

convulsions was more or less constant, be the dose large or small, while the surplus in the blood varied in proportion to the size of the dose administered.

It is then possible to answer part of the question which was asked at starting. The spinal cord in cases of strychnine poisoning certainly contains more strychnine than the other organs; whether or no it is more susceptible to the drug than the other organs we have no means of ascertaining.

The work has also a bearing in a medico-legal sense, inasmuch as it shows the storage power of the spinal cord and points to that as the place in which to search best for the drug in cases of poisoning where the stomach contents are not available. In this respect it forms an addition to the work of Professor Chittenden[1] on the "Significance of the Absorption and Elimination of Poisons in medico-legal cases."

[1] R. H. Chittenden, *Medico-legal Journal*, 1884.

PHYSIQUE OF WOMEN IN MASSACHUSETTS.

THE PHYSIQUE OF WOMEN IN MASSACHUSETTS.

By Prof. H. P. Bowditch, M.D.

In the eighth annual report of the State Board of Health of Massachusetts was printed an article entitled, "The Growth of Children." In this paper were embodied the results of a study of the height and weight of school children of Boston and vicinity, about 24,500 measurements having been made, chiefly upon pupils of the public schools of the city. Tables and curves were given, showing the average heights and weights of school children from five to eighteen years of age, of both sexes, and of parentage of various nationalities. In a subsequent article, two years later, the same data were further discussed, with a view to ascertaining the relative importance of race and environment in determining the rate of growth.

In order to enlarge the body of statistics from which conclusions could be drawn, the State Board of Health, Lunacy and Charity, in 1880, issued the following circular: —

Commonwealth of Massachusetts.

State Board of Health, Lunacy and Charity,
Department of Health, State House,
Boston, Jan. 1, 1880.

Dear Sir: — As a contribution to the vital statistics of Massachusetts, the State Board of Health, Lunacy and Charity desires to suggest the importance of collecting observations upon the physique of the inhabitants of the State. The usefulness of such an investigation will be evident from a consideration of some of the problems upon which it will throw light. These may be briefly enumerated as follows: —

1. The influence of geographical and climatic conditions on the growth of children and on the physique of adults.

2. The number of generations necessary for the complete development of the influence of changed climatic conditions on the physique of a given race.

3. The comparative effect of city and country life on the growth and development of the human race.

4. The relation between diseases and the rate of growth.

5. The effect of local hygienic conditions on the physique of children and adults.

That the results of the inquiry may have the greatest possible value, it is desirable that measurements should be made upon individuals of all ages and of both sexes. In pursuance of this object, the Board will be glad to co-operate with superintendents of public institutions, both charitable and penal, with persons having facilities for making observations in factories and mills, with school committees, with principals of academies and colleges, with fathers of families, and with all who are interested in this branch of scientific investigation.

To secure uniformity of method, the Board will distribute gratuitously, to all who desire to assist in this research, blank cards, on each of which are to be recorded the statistical data relating to a single individual.

If you desire to aid in this investigation, you are requested to state the number and sex of the persons in regard to whom you will be able to collect any or all of the above-mentioned statistical data. The Board will then gladly supply you with the necessary number of blank cards, and furnish you with full instructions as to the best method of taking and recording the measurements. The use of the metric system for this purpose is attended by so many and such decided advantages, that the Board is willing, if desired, to furnish certain simple forms of apparatus to facilitate the work. If, however, it is for any reason impossible to employ the metric system, measurements recorded in inches and pounds will be gladly received, and the conversion afterwards made at this office.

The statistics collected in this way will be placed in the hands of Prof. H. P. Bowditch, to whose articles on the growth of children in the eighth and tenth reports of the State Board of Health, investigators in this field of research are referred.

In behalf of the State Board of Health, Lunacy and Charity,

Very respectfully yours,

Charles F. Folsom, M.D.,
Secretary.

To those who expressed a willingness to aid in this investigation blank cards were forwarded, printed on both sides, as follows: —

FEMALES.] FOR A SINGLE SET OF OBSERVATIONS. [SEE THE OTHER SIDE.

Record all linear measurements at nearest centimeter; all weights at nearest kilogram.

Name (or initials), ______ *Age,* ______ *yrs.* ______ *mos.*

Height, without shoes, ______. *Sitting height,* ______. *Finger reach,* ______.

Chest girth, inspire, ______; *expire,* ______. *Weight,* (in ordinary in-door clothes.) ______.

Nationality of
- *Father,* ______. *Color of Eyes,* ______ *Color of Hair,* ______.
- *Mother,* ______. *Birthplace,* ______.
- *Paternal Grandfather,* ______. *Occupation (of husband if a married woman), (of parents if a minor).*
- *Paternal Grandmother,* ______.
- *Maternal Grandfather,* ______. ______.
- *Maternal Grandmother,* ______. *Name (or initials) of observer.*

(This card when filled is to be returned to Sec. of State Board of Health, Lunacy and Charity, State House, Boston, Mass.) ______

The Height is to be taken in an upright position, without shoes, the feet being close to the measuring rod. If, in the case of infants, it is necessary to measure in a recumbent position, the fact should be stated.

The Sitting Height is the vertical distance between the top of the head and the surface upon which the individual is seated.

The Finger Reach is the distance between the tips of the middle fingers when the arms are extended horizontally, the breast and arms being in contact with a wall.

The Chest Girth should be taken after a forcible inspiration and also after a forcible expiration, the measuring tape being passed horizontally round the chest on a level with the nipples, over only a single garment. This measurement is to be taken only on men and children.

The Weight is to be taken in ordinary in-door costume. In the case of children less than ten years of age, it is to be recorded at the nearest tenth of a kilogram.

The Color of the Eyes is to be recorded as blue, gray, brown or black.

The Color of the Hair is to be recorded as fair, golden, red, brown, black or gray. If gray, record also, if possible, the original color.

The Nationality is determined by the place of birth.

The Occupation should be given so as to indicate as far as possible the degree of comfort in which the individual lives.

See also article on "Anthropometrical Methods," tenth annual report Massachusetts State Board of Health, 1879, p. 55.

The advantages of this card method of collecting statistics are fully set forth in an appendix to the second of the above-mentioned articles.* To the explanations there given it need only be added that the short vertical lines, one centimeter apart, printed at the

* See tenth annual report of the Massachusetts State Board of Health, 1879, page 55.

top of the card, are intended to aid in marking the cards coming from any one institution or locality, so that they may be distinguished, when mixed with others, in sorting them with reference to any particular statistical inquiry. The marking was effected by screwing all the cards belonging to any one institution into a vise, and scoring the tops of them with a hand-saw at one or more of the vertical lines. A record being kept of the scored lines corresponding to each institution, it was always easy to identify the cards belonging to it.

The Board also authorized the construction of several sets of weighing and measuring apparatus which were sent to the different institutions in turn, in order to secure uniformity in the method of making the observations. By these means measurements were obtained in the following institutions, public and private, during the years 1881–85 : —

Name of Institution.	Number of Observations.		Name of Observer.
	Male.	Female.	
Wellesley College, . . .	–	302	Miss E. A. Nunn.
Mt. Holyoke Seminary, . .	–	203	A. A. Richardson, M.D.
Smith College, Northampton,	–	92	Lucy B. Hunt.
Normal School, Westfield, .	11	106	J. G. Scott.
Normal School, Bridgewater,	39	129	A. C. Boyden.
Normal School, Worcester, .	–	38	E. H. Russell.
Normal School, Framingham,	–	65	Miss E. Hyde.
Practice School, Framingham,	21	41	Miss E. Hyde.
Private School, Boston, . .	–	40	Miss Selma Wesselhoeft.
Private School, Boston, . .	–	35	Miss Ireland.
Private School, Boston, . .	–	25	Miss Gibbens.
Reform School, Westborough,	119	–	C. A. Robbins.
Reform School, Lancaster, .	–	70	N. Parker Brown.
Reform School, Lancaster, .	–	73	Miss Putnam.
Farm School, Boston, . .	97	–	F. M. Barrett.
Conservatory of Music, Boston,	–	127	S. L. Tourjée.
Almshouse, Tewksbury, .	37	200	Dr. Lathrop.
	324	1,546	

To the intelligent co-operation of the above observers the success which has attended the investigation is chiefly due.

The publication of the results of this research has been delayed in the hope that they might be embodied in a comprehensive treatise on anthropometry in Massachusetts ; but

the pressure of other duties has thus far prevented the preparation of such a work, and there seems to be no probability that in the immediate future such a task can be undertaken. Moreover, anthropometrical investigations have, in recent years, been carried on under the auspices of Harvard College, on a larger scale and in a much more thorough manner than was contemplated when these data were collected. The development of the human physique in Massachusetts is, therefore, a subject the discussion of which may properly be postponed till the data constantly accumulating in the hands of the Director of the Hemenway Gymnasium shall be numerous enough to enable him to draw positive conclusions upon the numerous questions to which his investition is directed. It has, therefore, been decided to present a few of the most obvious results derived from the study of the data thus far collected, and to point out some of the questions to the solution of which they may contribute in the hands of future investigators.

It will be observed that, in the above list, the observations on females are much more numerous than those on males. This was due to a special effort to obtain statistics relating to the female sex, which, in most anthropometrical researches, has been strangely neglected, though in all questions relating to the growth and development of the race its importance is at least equal to that of the male sex.

With the exception of the observations made at the Tewksbury Almshouse, which were set aside for special study, and have not been incorporated in any of the following tables, the great bulk of all the data were collected from individuals between the ages of seventeen and twenty-four years. Now, at the age of seventeen years, most girls have nearly completed their growth, as will be seen by a reference to the curves on Plate I in the above-mentioned article on the growth of children, in the eighth annual report of the State Board of Health. It therefore seemed possible, by means of the data at our disposal, to obtain a fairly correct idea of the physical type of the adult young woman of this community. One thousand one hundred and seven cards were found to contain data suitable for this determination, and attention was at first directed only to the record of the

height, weight, sitting height and finger reach, or stretch of arms, as it may more properly be called. As these last two measurements are interesting chiefly for the light they throw on the proportionate development of different parts of the body, their absolute values are less important than their relation to the total height of the individual. As a preliminary to tabulation, therefore, the absolute values of the sitting height and stretch of arms on each card were converted into percentage values of the total height. The cards were then treated by the method described in the appendix to the above-mentioned supplementary article on the growth of children, published in the tenth annual report of the State Board of Health.

Although, for the determination of the physical type of the adult young woman, the observations were to be used without regard to the age or the institution to which the individual belonged, yet, with a view to some future possible utilization of the statistics, the cards were at first sorted with reference to both these points. Thus the first tabulation showed, for each age and for each institution, the number of individuals observed, at each centimeter of height, each kilogram of weight and each half per cent. of sitting height and stretch of arms. Tables 1 to 4 give the result of this first tabulation after the observations had been added together, without regard to age or the institution in which the observations were made. Thus Table 1 shows that, out of 1,107 women of seventeen years of age and upwards whose height was measured, one was 139, one 141, four 143 centimeters high, and so on. From a table of this sort the average height is easily calculated, by multiplying each height in centimeters by the number of observations recorded at that height, adding the products together, and dividing the sum by the total number of observations. Average values for the dimensions recorded in the other tables are obtained in a similar manner.

Table 1.

Showing Distribution of Observations on Height of Women in Massachusetts (Seventeen Years Old and upward).

Height in Centimeters.	No. of Observations.	Height in Centimeters.	No. of Observations.	Height in Centimeters.	No. of Observations.	Height in Centimeters.	No. of Observations.
139	1	149	15	159	75	169	12
140	–	150	28	160	64	170	13
141	1	151	29	161	81	171	3
142	–	152	36	162	68	172	4
143	4	153	45	163	56	173	–
144	4	154	59	164	49	124	5
145	2	155	65	165	56	175	–
146	6	156	59	166	44	176	1
147	8	157	73	167	21	177	1
148	12	158	86	168	21		

Total number of observations, 1,107. Average height (without shoes) = 158.76 centimeters.

Table 2.

Showing Distribution of Observations on Weight of Women in Massachusetts (Seventeen Years Old and upward).

Weight in Kilograms.	No. of Observations.	Weight in Kilograms.	No. of Observations.	Weight in Kilograms.	No. of Observations.	Weight in Kilograms.	No. of Observations.
32	1	46	19	60	60	74	3
33	–	47	32	61	52	75	8
34	–	48	30	62	39	76	1
35	1	49	43	63	29	77	5
36	–	50	61	64	28	78	1
37	2	51	51	65	22	79	3
38	1	52	56	66	20	80	3
39	1	53	68	67	17	81	2
40	5	54	70	68	16	32	1
41	1	55	37	69	4	83	–
42	9	56	64	70	11	84	–
43	4	57	62	71	9	85	–
44	12	58	42	72	2	86	3
45	12	59	74	73	8		

Total number of observations, 1,105. Average weight (in ordinary in-door costume) = 56.51 kilograms.

Table 3.

Showing Distribution of Observations on Sitting Height as Percentage of Total Height of Women in Massachusetts Seventeen Years Old and upward.

Per cent. of Height.	No. of Observations.	Per cent. of Height.	No. of Observations.	Per cent. of Height.	No. of Observations.	Per cent. of Height.	No. of Observations.
37.5	1	42.5	–	49.5	2	55.5	49
38.	–	44.	–	50.	6	56.	28
38.5	1	44.5	–	50.5	23	56.5	10
39.	1	45.	–	51.	44	57.	8
39 5	–	45.5	–	51.5	60	57.5	1
40.	1	46.	1	52.	119	58.	1
40.5	1	46.5	–	52.5	111	58 5	2
41.	–	47.	2	53.	156	59.	–
41.5	–	47.5	1	53.5	177	59.5	–
42.	–	48.	–	54.	121	60.	–
42.5	–	48 5	–	54.5	101	60.5	1
43.	–	49.	4	55.	73		

Total number of observations, 1,106. Average sitting height = 53.24 per cent. of total height.

Table 4.

Showing Distribution of Observations on Stretch of Arms as Percentage of Total Height of Women in Massachusetts Seventeen Years Old and upward.

Per cent. of Height.	No. of Observations.	Per cent. of Height.	No. of Observations.	Per cent. of Height.	No. of Observations.
93.	3	98.5	34	104.	24
93.5	5	99.	30	104.5	22
94.	2	99.5	81	105.	15
94.5	3	100.	182	105.5	13
95.	8	100.5	118	106.	7
95.5	9	101.	56	106.5	3
96.	14	101.5	56	107.	5
96.5	10	102.	91	107.5	3
97.	26	102.5	88	108.	–
97.5	47	103.	46	108.5	1
98.	71	103.5	31		

Total number of observations, 1,104. Average stretch of arms = 100.54 per cent. of total height.

It will be observed that in Table 3 five of the observations are widely separated from all the rest, showing that five of the women who were measured had a sitting height from 37.5 to 40.5 per cent of their total height; while, with the exception of these five cases, the smallest percentage sitting height recorded was 46. This wide separation of a group of cases from the great mass of the observations suggests the influence of some abnormal cause of variation. It seems not improbable that in these cases some deformity (*e. g.*, spinal curvature) may have produced a great diminution in the sitting height; but, in the absence of any precise information on the subject, it has been thought best to include them in the table. Their effect upon the average percentage sitting height is inconsiderable. By rejecting them, this value is raised to 53.3 per cent., and thus becomes identical with the "median value," as will be shown below in Table 5.

It is evident that the arithmetical average represents but very imperfectly the series of observations from which it is calculated, since the same average value may be obtained from sets of observations differing very widely in their distribution. For instance, the two series of numbers 24, 25, 26, and 5, 25, 45, both give the average value 25. It is also evident that tables of distribution such as those above given are inconvenient, on account of their not being readily comparable with similar tables in which the total number of observations is different.

Various devices for overcoming these difficulties have been suggested by statisticians; but the scheme of "percentile grades" as proposed by Francis Galton, F.R.S., in 1885, and fully elaborated by him in his recent work on "Natural Inheritance,"* is perhaps better adapted than any other to display the results of a statistical inquiry, and to facilitate a comparison between various sets of observations. In this scheme are given values which are surpassed or unreached by various percentages of the total number of observations. In Table 1, for instance, five per cent. of the total number of observations (*i. e.*, 1,107) is 55.35. Now, in this table, since the heights are recorded at the *nearest* centi-

* Macmillan & Company, London & New York, 1889.

meter, it is evident that each successive group includes the observations between the half centimeter below and the half centimeter above the height recorded in the table. By adding together the numbers in the successive groups, we find that the sum of all the observations, at heights up to 149.5 centimeters inclusive, is 53; and, by adding the next group of 28 observations, the sum 81 is reached. Now, since 55.35 is between 53 and 81, it is evident that the height below which five per cent. of the observations fall is between 149.5 and 150.5 centimeters. The exact height can readily be calculated by interpolation. Thus the fraction of a centimeter which is to be added to 149.5 to give the required height, is obtained by dividing 2.35 (*i. e.*, 55.35 — 53) by 28 (*i. e.*, the number of observations at 150 centimeters). This fraction is 0.08; and therefore 149.58 centimeters is the height below which five per cent. and above which ninety-five per cent. of the observations fall. In a similar way, the heights corresponding to other percentages can be obtained, and a table constructed which presents in a very compact form the result of a large series of observations.*

Thus, in the first, fourth, seventh and tenth lines of Table 5 will be found the percentile distribution of the observations contained in tables 1, 2, 3 and 4 respectively; while in the second, fifth, eighth, ninth, eleventh and twelfth lines are introduced, for purposes of comparison, the results of similar observations made by Dr. D. A. Sargent,† director of the Hemenway Gymnasium, upon individuals of both sexes, in this community; and in the third and sixth lines are given the figures obtained by Galton‡ in his measurements of English women at the anthropometric laboratory in the international exhibition of 1884. In the lines numbered 1*a* to 6*a* the values given in the first six lines in centimeters and kilograms are reproduced in inches and pounds, to facilitate comparison with tables in which the English weights and measures are used.

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* A geometrical method of determining the values corresponding to the various percentile grades is given by Francis Galton in an article on "Anthropometric Percentiles," Nature, Vol. 31, page 223.

* Scribner's Magazine, Vol. 172.

† Natural Inheritance, London, 1889, page 200.

Table 5.

	Subject of Measurement.	Name of Observer.	Age.	Sex.	No. of Observations.	Unit of Measurement.	Values at the Under-mentioned Percentile Grades.										
							5 Per Cent.	10 Per Cent.	20 Per Cent.	30 Per Cent.	40 Per Cent.	50 Per Cent.	60 Per Cent.	70 Per Cent.	80 Per Cent.	90 Per Cent.	95 Per Cent.
1	Height (without shoes), ...	Bowditch, .	17 years and upward,	F.	1,107	Centimeters, .	149.58	151.52	154.01	155.78	157.44	158.77	160.37	161.82	163.67	165.82	167.72
2	Height (without shoes), ...	Sargent, .	16–26 years, . .	F.	1,835	Centimeters, .	150.1	151.9	153.9	155.9	157.5	159.1	160.5	162.0	164.1	166.6	168.4
3	Height (without shoes), ...	Galton, .	23–51 years, . .	F.	770	Centimeters, .	149.5	152.2	155.7	157.7	159.2	160 7	162.2	164.0	165.7	168.7	171.0
4	Weight (in ordinary in-door clothes), .	Bowditch, .	17 years and upward,	F.	1,105	Kilograms, . .	45.82	47.85	50.28	52.33	53.97	56.07	58.00	59.75	61.85	65.82	69.93
5	Weight (without clothes), . . .	Sargent, .	16–26 years, . .	F.	1,901	Kilograms, . .	42.0	44.0	46.3	49.0	50.4	52.0	54.0	55.4	58.6	62.2	65.8
6	Weight (in ordinary in-door clothes), .	Galton, .	23–26 years, . .	F.	276	Kilograms, . .	46.3	47.7	49.9	51.7	53.6	55.4	58.6	59.9	61.7	64.5	67.6
7	Sitting height, ...	Bowditch, .	17 years and upward,	F.	1,106	Per cent. of height,	50.87	51.43	52.05	52.54	52.95	53.30	53.61	54.01	54.51	55.17	55.70
8	Sitting height, ...	Sargent, .	16–26 years, . .	F.	about 2,000	Per cent. of height,	50.6	51.1	51.6	52.1	52.4	52.7	53.1	53.4	53.9	54.4	54.9
9	Sitting height, ...	Sargent, .	16–26 years, . .	M.	about 2,000	Per cent. of height,	50.2	50.5	51.1	51.6	52.0	52.2	52.6	52.9	53.3	53.8	54.3
10	Stretch of arms, ...	Bowditch, .	17 years and upward,	F.	1,104	Per cent. of height,	96.77	97.57	98.58	99.68	100.02	100.36	100.92	101.83	102.46	103.47	104.79
11	Stretch of arms, ...	Sargent, .	16–26 years, . .	F.	about 2,000	Per cent. of height,	97.0	97.9	98.8	99.7	100.3	100.8	101.3	101.9	102.9	103.8	104.5
12	Stretch of arms, ...	Sargent, .	16–26 years, . .	M.	about 2,000	Per cent. of height,	99.1	100.0	100.9	101.7	102.2	102.8	103.4	104.0	104.8	106.0	106.8
1 a	Values of line 1 converted into, ...					Inches, . . .	58.9	59.7	60.6	61.3	62.0	62.5	63.2	63.7	64.5	65.3	66.0
2 a	Values of line 2 converted into, ...					Inches, . . .	59.1	59.8	60.6	61.4	62.0	62.6	63.2	63.8	64.6	65.6	66.3
3 a	Values of line 3 converted into, ...					Inches, . . .	58.9	59.9	61.3	62.1	62.7	63.3	63.9	64.6	65.3	66.4	67.3
4 a	Values of line 4 converted into, ...					Pounds, . .	100.9	105.0	110.8	115.3	119.0	123.7	127.9	131.7	136.2	145.0	154.1
5 a	Values of line 5 converted into, ...					Pounds, . .	92.6	97.0	102.0	108.0	111.0	114.6	119.0	122.0	129.0	137.0	145.0
6 a	Values of line 6 converted into, ...					Pounds, . .	102.0	105.0	110.0	114.0	118.0	122.0	129.0	132.0	136.0	142.0	149.0

The figures in the column headed 50 per cent. give the measurements which are exceeded by one half and not reached by the other half of the individuals measured. These values are termed by Galton "median values" (or M) and are considered by him (at least in the observations which he discusses) as practically the same as the arithmetical means or averages.* It will be observed that in Table 5 the median values do not differ materially from the average values given in tables 1, 2, 3 and 4. The median and the average values of the height and sitting height are almost identical; while in the case of weight and stretch of arms, the median values fall somewhat short of the averages.

It is evident that, when the values at the lower percentile grades fall short of the median value by *the same amount*, the values at the higher percentile grades exceed it (or, in Galton's words, when "the curve of the scheme is symmetrically disposed on either side of M"), the median and average values will be identical. When, however, the *lower* percentile values fall short of the median value *more* than the higher percentile values exceed it, the average will be less than the median value; while a difference in the opposite direction will cause the average to exceed the median value. The difference between the median and the average value, or, as we may express it, the value M — A, becomes, therefore, a convenient indication of the direction and extent of the asymmetry of the curve of percentile distribution.

The interesting results which may be expected from an application of this method of discussion to the data already collected relating to the growth of children, will be presented in a future article.

An important feature in a table of percentile distribution, like Table 5, is found in the facility which it affords for determining the rank of an individual among others of the same class. Suppose, for instance, that a woman belonging to the class whose heights are given in the first line of Table 5, desires to know how her height compares with that of the other women measured. Suppose her height to be 160 centimeters. The table shows at once that this height exceeds

* Natural Inheritance, page 41.

that of 50 per cent., but does not exceed that of 60 per cent., of the women measured; and a simple sum in proportion shows that 160 centimeters corresponds to about 57.7 per cent. In other words, the woman in question, if ranked according to height, would stand about 577th in a group of 1,000 women selected at random.

Let us now consider what conclusions can be drawn from a comparison of the measurements described in this paper with those of other observers, as recorded in Table 5.

Height.

It will be observed, in the first place, that the heights recorded in the first line are slightly inferior to the corresponding measurements by Dr. Sargent, as given in line 2; while the figures in line 3, given by Mr. Galton as the result of his measurement of English women, are somewhat in excess of those obtained on this side of the Atlantic. In explanation of these differences, it may be said that the American measurements, having been taken upon women as young as sixteen or seventeen years, probably include a certain number who, being of slow growth, have not yet attained their full height; while the English measurements, being limited to women between twenty-three and fifty-one years, represent more accurately the adult height of the female portion of the community. It is also possible that we are dealing here with a difference of race, though the close correspondence which has been shown to exist between the heights of growing boys of the two nationalities does not support this view.

It is, furthermore, interesting to note that the difference between the heights at the fifth and the ninety-fifth percentiles is greater for the English than for the American women; being 21.5 centimeters for the former and 18.1 centimeters for the latter. It will be convenient to term this difference the *range* of the heights, though it of course indicates the range of only nine-tenths of the whole number of observations, the highest and lowest 5 per cent. being omitted. It will also be observed that the difference in height between the women of the two nationalities increases with tolerable regularity, as we ascend the series of percentile grades, from — 0.8 to 3.28 centimeters.

The slight excess of Dr. Sargent's measurements over our own is probably to be explained by the fact that the former were taken in schools frequented by the children of the most favored classes, while the latter include a certain number of individuals who had not grown up in the midst of comfortable surroundings.

Weight.

The weights given in the fourth line of Table 5 are seen to be considerably in excess of Dr. Sargent's figures in line 5, owing to the fact that the latter represent net weights, while our own observations were made upon women "in ordinary in-door clothing." The excess at most of the grades amounts to 3.3–4.0 kilograms (8–9 pounds), — a difference which corresponds very well to the weight of clothing of the older girls, as given in Table 25 of the article on the growth of children, already alluded to.

A more accurate comparison may be made between our own observations and those of Mr. Galton (line 6), since the latter were also made upon individuals in ordinary in-door clothing. It will be observed that the range of the weights is greater in American than in English women, there being between the fifth and the ninety-fifth percentile grades a difference of 24.1 kilograms for the American and 21.4 kilograms for the English women. It will be also noticed that at most of the percentile grades the weight of the American exceeds that of the English women, the difference being most marked in the highest percentiles. This observation, in connection with the one above noted with regard to heights, seems to show that there is little difference between the shortest as well as between the lightest women of the two nationalities; but that the tallest English women surpass the tallest American woman in height, while the heaviest American women exceed in weight the heaviest English women. Before this conclusion can be accepted as absolutely established, it will be necessary to determine how far the greater age of the English women has affected the result. In other words, we must ascertain more accurately than is at present possible, at what age the growth of women, both in height and weight, can be regarded as completed.

Sitting Height.

A comparison of the figures in the seventh and eighth lines of Table 5 shows that the ratio of the sitting height to the total height is somewhat greater in our measurements than in those of Dr. Sargent, the median value being 53.3 per cent. in the former, and 52.7 per cent. in the latter. This difference is doubtless associated with the superior height of the women measured by Dr. Sargent, in a manner which will be presently explained. For purposes of comparison, Dr. Sargent's measurements of the sitting height of men are given in the ninth line of Table 5; and it is interesting to notice that they are lower than the corresponding measurements of women at all the percentile grades. In other words, women appear to be relatively longer in the body and shorter in the legs than men. Whether this is a sexual peculiarity, or whether it depends upon the fact that men are, as a rule, taller than women, is a question which can be settled only by a comparison of the percentage sitting height of a large number of men and women of the same total height.* It is a good illustration of the readiness with which the card method of recording statistics lends itself to the solution of problems which may arise subsequently to their collection, that the data already on hand can be made to contribute to the settlement of the question thus suggested.

In the first place, in order to obtain a set of statistics as comparable as possible with those of Dr. Sargent, those observations only were selected for discussion which were made in schools and colleges on women of seventeen years of age and upwards. Ten hundred and fifty-eight cards were found to contain records of this sort. These cards were then sorted according to the height of the individuals, the measurements falling within each successive set of 5 centimeters being brought into a single group. The cards in each group were then sorted according to the percentage sitting height, as shown in Table 3, and the percentile distribution of the observations in each group calculated in the manner above described. The results of this calculation are given in Table 6.

* Ranke (Beiträge zur Anthropologie und Urgeschichte Bayerns VIII. 56) regards the relatively greater length of the body in women as a proof that the female sex stands embryologically on a lower level than the male.

TABLE 6.

Table Showing Percentile Distribution of Observations on Ratio of Sitting Height to Total Height in Women of Seventeen Years and upward, arranged in Groups according to Total Height.

	Height, Centimeters.	No. of Observations.	Values at Undermentioned Percentile Grades.										
			5	10	20	30	40	50	60	70	80	90	95
1	145–149	43	52.29	52.80	53.34	53.58	54.05	54.44	54.68	55.06	55.49	56.03	56.67
2	150–154	184	51.59	52.20	52.79	53.19	53.51	53.81	54.09	54.41	54.81	55.34	55.81
3	155–159	343	51.29	51.67	52.14	52.78	53.13	53.53	53.88	54.16	54.65	55.21	55.65
4	160–164	303	50.70	51.34	52.03	52.49	52.86	53.10	53.34	53.56	53.91	54.56	54.99
5	165–169	155	50.52	50.89	51.36	51.84	52.10	52.35	52.59	53.00	53.49	54.03	54.50
6	170–174	23	47.32	48.20	50.45	50.99	51.31	52.08	52.43	52.66	53.31	53.70	54.17

An examination of this table shows that the ratio of the sitting height to the total height at all the percentile grades decreases as the total height increases. In other words, tall women differ from short women less in the length of the body than in the length of the legs. That this is also true of men, there is no reason to doubt; but whether the ratio in question varies with the sex as well as with the height of the individual, is a question to be settled by a comparison of the values in Table 6 with those to be obtained by a similar discussion of Dr. Sargent's observations on men. In his dis-

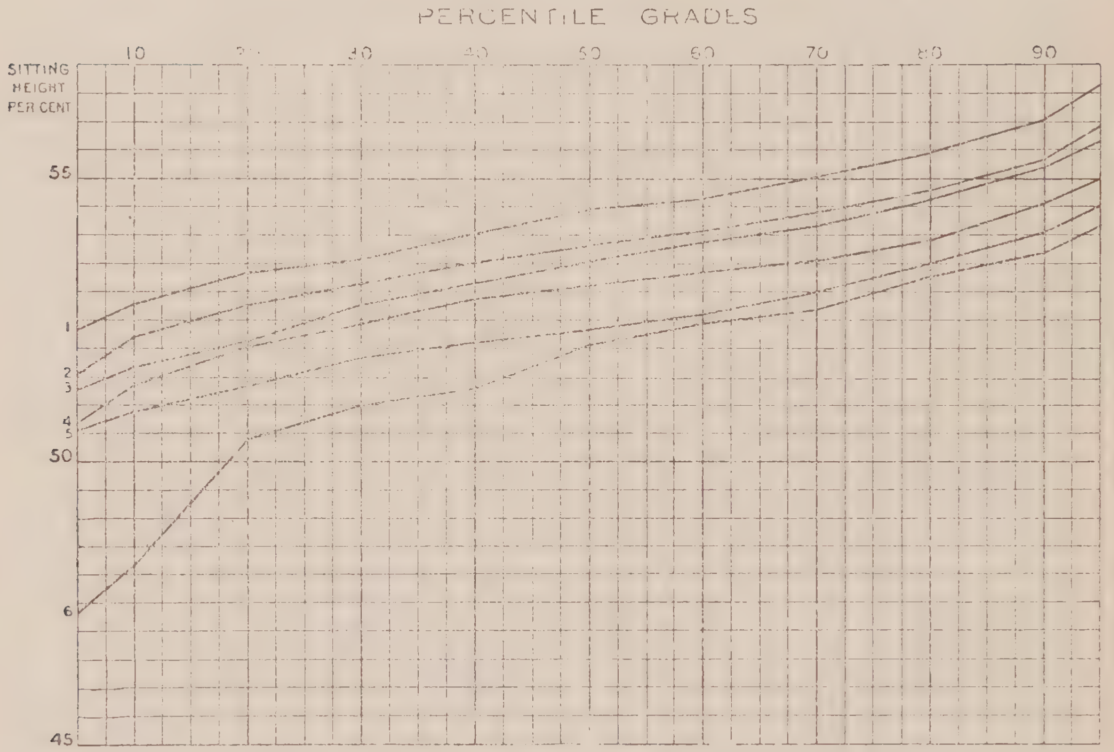


FIG. I CURVES CONSTRUCTED FROM TABLE 6.

cussion of the anthropometrical differences between boys and girls, Dr. Sargent, in the above-mentioned article, points out that, at the age of fifteen years, boys are superior to girls in height, mainly on account of the greater length of the thigh bone. It would be of great interest to determine, by a further discussion of his observations in the manner above indicated, whether a disproportionate increase in the length of this portion of the body is mainly responsible for the differences in height between individuals of the same sex.

The way in which the ratio of the sitting height to the total height varies with the total height, may perhaps be

better understood from an examination of the curves in Fig. 1, which have been constructed from the figures in Table 6. The abscissas represent the successive percentile grades, and the ordinates the corresponding values of the ratio. It will be observed that the curves, which represent in order from above downward the values in the successive lines of the table, are in a general way parallel to each other; with the exception, however, of the lowest curve of all, which, being constructed from only twenty-three observations, is, of course, much less accurate than the others. These curves show very clearly, that, with an increase of height is generally associated a relative diminution of the sitting height, and thus furnish a sufficient explanation of the fact above referred to, — that Dr. Sargent's measurements give a lower relative sitting height than our own. They show also, that, when the observations are arranged in groups according to the value of the total height, there is in each group a large range in the value of the relative sitting height; so that a certain number of women in the tallest group sit relatively higher than certain other women in the shortest group. For instance, a relative sitting height of 53.3 per cent. is surpassed by 20 per cent. of the women in the tallest group, while it is not reached by 20 per cent. of those in the shortest group.

Stretch of Arms.

It is popularly supposed that the distance between the finger tips when the arms are horizontally outstretched, is equal to the total height. An examination of the figures in the tenth, eleventh and twelfth lines of Table 5 shows that this is much more nearly true for women than for men; the median values in Dr. Sargent's observations being 100.8 per cent. for women, and 102.8 for men. Our own observations give at nearly all percentile grades slightly lower values than those of Dr. Sargent, — a difference which it seems reasonable to associate with the somewhat superior height of the women measured by Dr. Sargent; for, as we have just seen, tall individuals differ from short ones more in the length of the legs than in that of the body, and length of arms is generally associated with length of legs. A dis-

cussion, similar to that of the sitting height as above given, showing how the ratio of the arm-stretch to the height varies with the height, would, of course, settle the question; and a further discussion of Dr. Sargent's observations would also determine how far the superior ratio of the male sex is dependent upon a greater length of arms, and how far the greater breadth of the shoulders contributes to the result. In both sexes it will be observed there is a range of 7 or 8 per cent. between the fifth and ninety-fifth percentile grades.

It is evident that the data already on hand may be made to contribute to the solution of various questions connected with the physical development of the human race in Massachusetts; and that, with a further increase of the body of statistics, light will be thrown upon a variety of anthropological problems, especially those relating to the effect of race and environment on the physique of the individual. The systematic collection of such statistics in the public institutions of the State, would, in a few years, furnish data of inestimable value for the hygienist and the educator, as well as for the anthropologist and the statesman.

In conclusion, I desire to express my thanks to Miss Mary P. Nichols and to Miss Lucy R. Bowditch for much valuable assistance in the computation of tables.

THE MOVEMENTS OF THE LOWER JAW.

BY CHARLES E. LUCE, HARVARD DENTAL SCHOOL.

This paper presents the results of certain original investigations conducted in the physiological laboratory of the Harvard Medical School under the direction of Prof. Henry P. Bowditch. The results obtained are interesting, as they indicate the exact movements of the jaw, and prove that certain errors exist in all descriptions of this articulation as given by the eminent anatomists and physiologists. These discrepancies may be briefly noted as follows: —

Monro[1] wrote, "that the mouth could not be opened, if the lower jaw was protruded, without withdrawing it from its advanced position;" this is clearly incorrect, as will be indicated later.

Ferrein[2] was quite accurate in his description, but he wrote that "the condyle advances under the eminence;" in many cases it goes under it and mounts the other side, which he omitted to say.

Humphrey[3] falls into the same error and said that "the condyle advances upon the glenoid ridge and should not go quite to the summit," which in many cases it certainly does.

Morris[4] was in error when he wrote that the condyle itself never reaches quite so far as the summit of the glenoid ridge.

Küss[5] wrote that the lower jaw, as it rises and falls, represents a lever moving around a supposed axis centred at the condyle, which remains in the glenoid cavity in small openings; and in greater separation the supposed axis is placed at, or near, the dental foramen; this is also incorrect, as will be proven later.

Quain[6] states "that the condyle rests on the convex root of zygoma when the mouth is opened. As stated above, in most cases it advances farther forward than he states.

The error of Gray[7] is in the statement that in openings of slight extent, the condyles simply rotate on a transverse axis against the cartilages, whereas

1 Medical Essays, Edinburgh, 1735.
2 Collection Academique, Paris, 1785.
3 Humphrey's Human Skeleton, 1858.
4 Anatomy of the Joints, Morris.
5 Lectures in Physiology, Küss, Duval.
6 Quain's Anatomy, 1883.
7 Gray's Anatomy, 1887.

the condyles begin to move forward simultaneously with the beginning of opening. Again, he says the condyles simply "glide on to the articular eminence."

The first requisite in the study of jaw movements is to move the jaw and get a permanent record of the movement; the method used was the photographic, the same as that used by Marey and others, and may be described as follows: a bright silver bead was fastened to a wooden pin or dowel, which was firmly inserted between the inferior central incisors; with the subject in a strong sunlight, so that a bright spot should be reflected from the bead, a pure profile or side view was photographed, and the sensitive plate was exposed during the opening of the mouth; the bright spot reflected from the bead during the motion was continuously photographed and its excursion recorded on the negative as a line, giving the actual movement of the place upon the jaw to which the bead was opposed.

The earlier experiments dealt solely with the simple tracing at the symphysis, and while the results obtained with one bead were both instructive and interesting, the more valuable results were found by getting the relative movements of condyle,

Fig. 1.

angle, and symphysis; to get tracings at these points, a light framework was constructed, which

simply reached around the face from the lower incisor teeth, to which it was securely fastened, nearly to the ear; adjusting devices held bright beads which could be placed directly opposite the condyle, angle, and symphysis; the photographing took place as before, each bead making its tracing.

Figure 1 shows the device as applied to a skull; the pin is inserted between the lower front teeth, two of the bright beads being adjusted directly opposite the condyle and angle, the other in front of the symphysis. It is apparent that the jaw and device will move in concert, and that the movement of the beads is, in fact, the same as that of the points opposite to which they are placed.

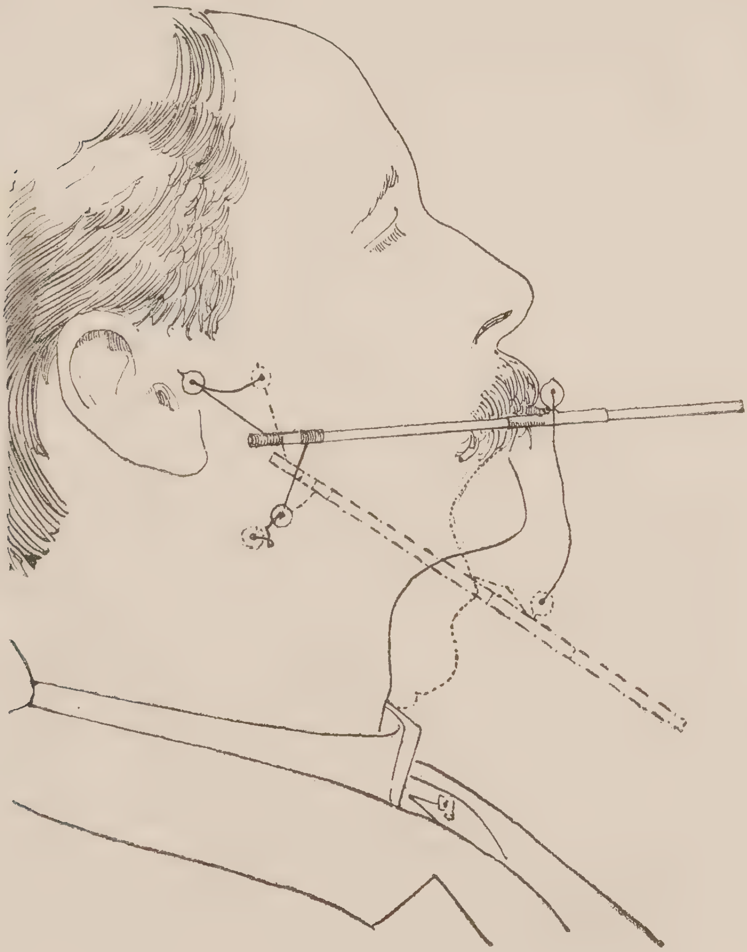


FIG. 2.

Figure 2 represents the tracings as taken at the three points mentioned.

It now becomes necessary to indicate the methods by which the several tracings were subsequently studied. The picture was enlarged in outline by optical projection, and the tracing of the opening at symphysis was subdivided into convenient spaces; then, by means of dividers, corresponding points

in the tracings at condyle and angle were determined as follows.

Referring to Figure 3, it is evident that the distance between the point marked 0 in the tracing at symphysis and the point marked 0 at the condyle does not change, inasmuch as both points are attached to a device which does not change its length; and though these points may seek different positions when the mouth is opened, the distance between them remains the same; and we find the ends of the tracings at 8 to be the same distance apart as at 0, the beginning. If the dividers are set at the proper distance, we can find, by simple measurement, how far the condyle moves in a given movement at the symphysis, and the motion of the angle is worked out in the same manner. The numbers on the tracings in Figure 3 indicate

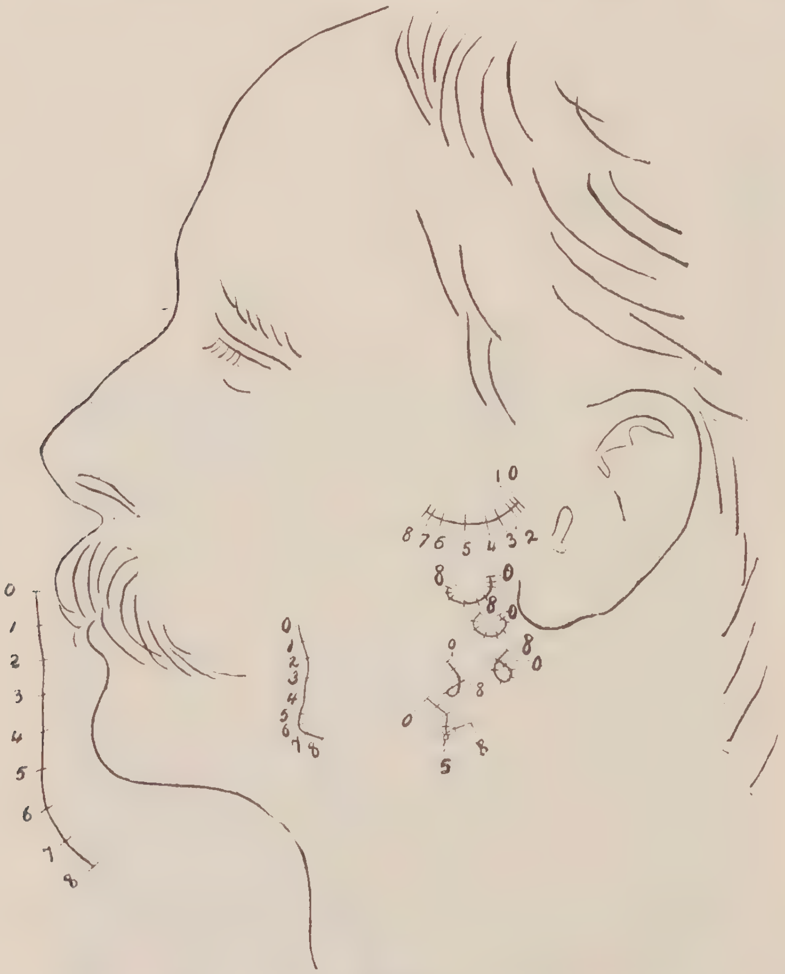


Fig. 3.

the positions occupied simultaneously by the various points of the jaw, the movements of which have been studied.

The tracings at all points in the jaw are readily understood, if we regard their motion as the result

of a combination of a uniform downward and backward rotation round the condyle, with a forward motion of the condyle itself in a curved line, with the concavity upwards, always bearing in mind that this movement of the condyle is, at first, slow, then more rapid and subsequently slow again. It is evident that points in the immediate neighborhood of the condyle will have a movement differing very little from that of the condyle itself, while at more distant points the backward rotation round the condyle will be relatively more prominent, because the motion is on the circumference of a larger circle.

The tracings in Figure 3 illustrate this point. It will be noticed that the tracing nearest to that of the condyle is, like the latter, a curve with its con-

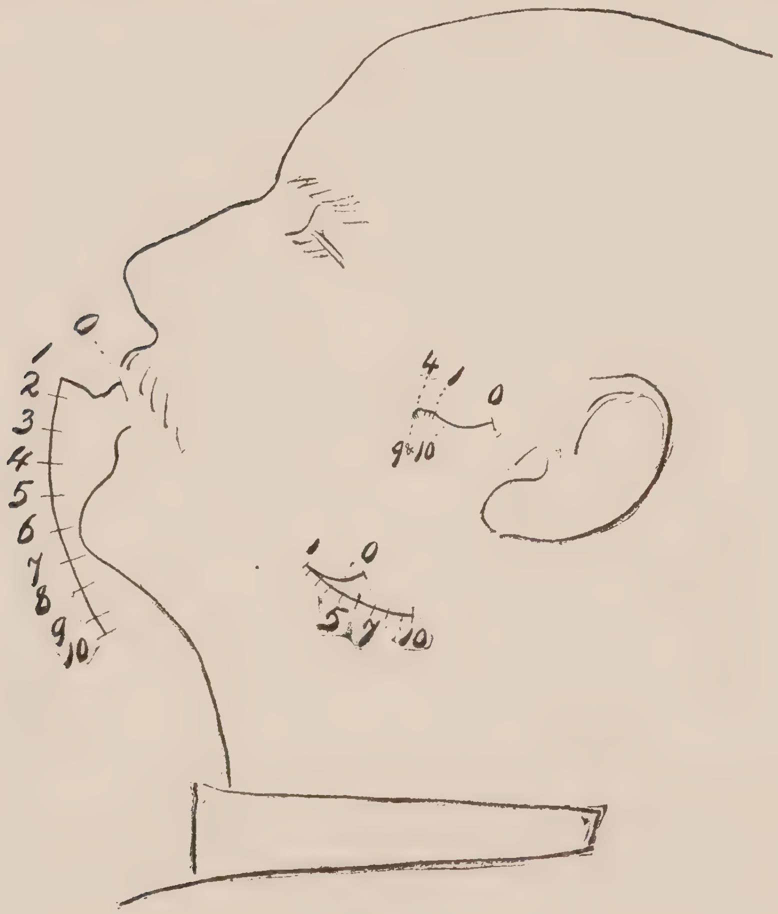


Fig. 4.

cavity upward, but at the beginning and end of the motion, where the movement of the condyle is relatively slow, the rotation round the condyle is able to impress its backward motion upon the tracing. In studying the motion of points in the ramus, successively more and more distant from the condyle, we observe that the backward rotation becomes

more and more prominent as a factor, and the forward movement due to the gliding of the condyle round the articular eminence less and less marked. The fact that this forward movement is most rapid in the middle of its course explains the loop-like character of these tracings, which is retained even so far down as the angle of the jaw.

By referring to Figure 3, it will be noticed that the condyle begins to move forward immediately, and even in a small opening of the mouth it performs quite a considerable excursion, contrary to the assertion of Gray and others to the effect that

FIG. 5.

in small openings the condyles simply rotate on a transverse axis against the fibro-cartilages.

Again, it is stated by Morris, Humphrey, and others that the condyle never quite reaches the summit of the eminence; most of the tracings show that it does reach the summit and even begins to mount the anterior side, as may be seen by Figure 3.

There are, however, exceptions, and it seems necessary that any assertion as to the action of this articulation should be qualified by the statement that there is considerable individual variation in the relative movements of condyle, angle, and symphysis.

The idea of Meyer[8] that the jaw is suspended or hung in its lateral ligaments is substantially correct, and we can readily see that in moderate opening, when the condyle advances, the movement of the angle is comparatively slight, but when the capsular ligament becomes tense the condyle is kept from farther anterior movement and the angle goes back, as may be seen in Figure 3, the jaw swinging from the attachment of the lateral ligaments on the temporal bone.

Monro, as I have previously stated, said that if the jaw was protruded it would be impossible to open the mouth without the jaw sliding back, or a dislocation occurring, and Ferrein disputed this statement.

Figure 4 vindicates the Frenchman, inasmuch as it shows that after protrusion, when the mouth was opened the condyle continued to advance, contrary to Monro's theory.

Figure 5 is a tracing of a subject who tried to keep the condyle back while opening the mouth, and it is shown, by comparison with the normal opening in Figure 3, that the condyle was held back in a measure, but the interesting point in this connection is that although Gray asserts that the condyle simply turns on a transverse axis in the cartilage in small openings, this tracing proves that the condyle advances even if an effort is made to hold it back.

Other tracings were taken of the down and up motion of the jaw with the subject facing the camera. While the sensitive plate was being exposed the mouth was opened, the bead making its tracing, and when the mouth was opened widest the head was turned slightly, that the tracing of the bead in closing should not be in contact with the opening tracing.

The lines drawn show that the jaw is not depressed or closed in a straight line; the tracings taken contain several curves, which will vary with the same person at different times, on account of

[8] Die Statik und Mechanik des Menschlichen Knochengerüstes. Leipsig, 1873.

the extreme mobility of this articulation and the fact that the condyles do not advance synchronously.

These tracings are interesting to dentists, inasmuch as they show what has to be contended against in taking the "bite" preparatory to the insertion of an artificial denture.

FIG. 6.

To conclude, as Ferrein said one hundred and fifty years ago, the movements of the lower jaw are not well understood, and much that is written of them is at least faulty. Probably some inaccuracies may be explained by the fact that anatomists have made too much use of the cadaver to demonstrate the actions of the articulations, whereas it may be that the contraction of the various muscles co-operating with and antagonizing each other is the important factor in determining the exact movements of the jaw.

If these investigations throw any light upon the movements of the lower jaw, or awaken an interest in the further study of them, something will have been accomplished.

Extracted from the
American Journal of the Medical Sciences for January, 1888.

THE ANTI-BACTERIAL ACTION OF IODOFORM.[1]

By J. Amory Jeffries, M.D.,
of Boston.

For some years iodoform has been in general use as an application for all sorts of sores, ulcers, and wounds, and regarded as a reliable germicide. This quality of iodoform has been questioned—indeed, its value in surgery denied—by Heyn and Rovsing.[2] Since then, quite a war has been waged in Germany, between the believers in the value of iodoform and their antagonists. To contribute certain points toward the solution of this question is the object of this paper.

The early literature bearing on the subject is meagre to a degree. Mikulicz[3] made two sets of experiments with various nutritive fluids, among them urine and a half and half solution of blood and water. In the first set one portion of fluid was passed through a filter covered with a layer of iodoform, and the other portion through a like filter without iodoform. Both sets of fluid were then placed in a warm chamber, and the changes noted from day to day. The parts passed through the iodoform showed slightly less turbidity and remained longer free from foul odors than the others. The difference was most marked in the blood mixture, which also had the strongest odor of iodoform. In the second set of experiments, Mikulicz simply added iodoform to half of each fluid and then treated as before. He found the parts containing iodoform showed retarded cloudiness, and the blood, even after ten days, was not foul-smelling, but aromatic.

Renno[4] experimented as regards the virulence of pus, from two abscesses, mixed with iodoform; in one case with no result, in the other with an apparent slight diminution in activity.

Lastly, Behring[5] studied the question and came to the following conclusions: Blood is the only healthy tissue that frees iodine from iodoform; iodine is set free by putrefaction and by stinking wounds. Hence, iodoform is inert in a dry wound, except putrefaction occurs.

1 The work for this paper was done in the bacteriological laboratory of the Harvard Medical School, according to the rules laid down at the Hygienic Institute at Berlin.

2 Heyn Chr. und Rovsing Thorkild: Das Iodoform als antisepticum. Fortschr. der med., 1887 Bd. v. No. 2, S. 33–47.

3 Mikulicz: Langenbeck's Archiv f. klin. Chirurgie, Bd. xxvii. Heft 1, 1881, S. 196–239.

4 Renno: Centralbl. f. klin. med., 1883, No. 50.

5 Behring: Ueber Iodoform und Iodoformwirkung. Deutsch. med. Wochenschr., 1882, No. 9, S. 146–148.

Looking over the above articles, it at once becomes apparent that they are very inconclusive ; nothing else could be expected, as bacteriology did not exist and the causes of suppuration were unknown. Renno's observations are too few for any deductions ; Mikulicz's only touch putrefaction as tested by transparency and odor; while Behring, by assumption, classes all pathogenic bacteria as putrefactive. Yet, all point to some action where putrefaction is going on.

We now come to the recent and more extensive literature. Heyn and Rovsing made a series of experiments in the laboratory with pure cultures of staphylococcus pyogenes aureus and a few ill-determined species. All the experiments were made in accordance with the accepted rules, and are free from errors of method. They are divided into the following series: Gelatine tubes inoculated on the surface and then coated with iodoform ; gelatine and agar-agar tubes mixed with iodoform and then inoculated; infected ligatures exposed to the action of four per cent. iodoform oil and then planted; iodoform mixed with blood serum and then inoculated; bacteria mixed with dry iodoform and planted ; gelatine exposed to an iodoform spray ; cultures from an iodoform tampon, and, lastly, the injection of cultures of staphylococcus pyogenes aureus and iodoform into rabbits. In every case the result was the same as in the control experiments conducted the same way, save for the omission of the iodoform. Hence the authors conclude that iodoform is no antiseptic, and is not indicated in surgery as at present applied.

Slightly prior to Heyn and Rovsing, Lübbert[1] published a monograph on staphylococcus pyogenes aureus, one set of experiments in which coincides with and confirms those of Heyn and Rovsing.

Objections at once came in from all sides, but since they are not based on experiments or careful observations, they may be summarized as follows: that laboratory tests are no evidence of what occurs in the body ; that in these tests, no iodine, by virtue of which iodoform acts, was set free. Strangely enough, none have pointed out that the experiments only disprove a germicidal action, but do not exclude an inhibitory action on their growth. The injurious effects of crowding are so great, in solid cultures, as soon to cover up any slight inhibitory action of the iodoform.

Next Rovsing,[2] in continuation of his first set of experiments with Heyn, inoculated the anterior chambers of the eyes of three rabbits with a mixture of one part of tubercle rubbed up with five parts of iodoform. In two more rabbits he inoculated one eye with the mixture, and the other with pure tubercle. Tuberculosis developed in every

[1] Lübbert: Biologische Spaltpilzuntersuchung. Der Staphylokokus pyogenes aureus und der Osteomyelitus kokkus, S. 109, Mit 2 Taf. Würzburg, 1886.

[2] Rovsing: Hat das Iodoform eine antituberculöse Wirkung. Fortschr. der med., 1887, Bd. v No. 9, S. 257–266.

case. By these experiments Rovsing claims that the specific anti-tuberculous action of iodoform is disproved.

Shortly Lübbert[1] returned to the subject, and reported the results of injecting a mixture of staphylococcus pyogenes aureus and iodoform under the skin of rabbits, in pockets under the skin, and in muscle wounds; all gave the same results as the control experiments conducted without iodoform. He also injected iodoform ether, and then the bacterium into the knee-joint, with positive results.

Sattler[2] experimented with silk threads saturated with staphylococcus pyogenes aureus, dusted with iodoform, and then laid on gelatine plates. He found that the growth of the coccus from threads thus treated was decidedly slower than from threads which had not been exposed to the action of iodoform. No control experiments with inert powders are reported.

About the same time Behring[3] claimed that iodoform only acted, when decomposed, in virtue of acetylen being set free.

Since the work of my own paper was completed, an article by Sänger[4] has come to hand, which contains two claims of importance: first, that the presence of non-growing anthrax bacilli causes gelatine tubes to dissolve; second, that iodoform limits the growth of the anthrax bacillus, and also prevents their taking in a reinoculation.

Thus, while the early authors noted a distinct action on putrefaction, the more recent writers have failed, for the most part, to note any action on the pus-producing bacteria. On the other hand, the mass of opinion from clinicians is in favor of the drug. Here is an antagonism of opinion that should not exist; clinical experience and laboratory experiments should lead to the same result; if not, one or both must be wrong. The evidence from neither one nor the other can be blindly accepted or safely ignored. To discover a way out of the dilemma is the object of this paper.

The first set of experiments was made in the same way as those of Heyn and Rovsing, to verify or disprove them, and, if possible, to discover a plan or suggestion for further experimentation.

A. To five test-tubes of melted gelatine was added five per cent. of iodoform, and the mixture solidified while rotated in ice water, so as to catch the iodoform in suspension. The five were then inoculated with my experiment bacillus. My experiment bacillus was derived from water, is long, rapidly moving, and dissolves gelatine.

2d day. All five tubes growing.

[1] Lübbert: Ueber das Verhalten von Iodoform zum Staphylococcus pyogenes aureus. Fortschr. der med., 1887, Bd. v. No. 11, S. 330–345.

[2] Sattler: Sitzungsbericht d. Deutsch Opthal., 1883, S. 89 u. 98.

[3] Behring: Ueber Iodoform und Acetylen. Deutsch. med. Woch., 1887, No. 20.

[4] Sänger: Ueber die Einwirkung des Iodoforms auf das Wachsthum und die Virulenz der Milzbrandbacillen. Deutsch. med. Woch., No. 331, 887, S. 726–728.

14th day. All five tubes completely dissolved. Fresh tubes inoculated from the above gave a good growth. No difference noted from the pure gelatine cultures made at the same time.

B. Five tubes of gelatine were solidified on the slant and inoculated with a line of staphylococcus pyogenes aureus, after which the whole surface was covered with a layer of iodoform, five millimetres thick.

3d day. Three of the tubes have taken, two show no signs of growth.

10th day. One of the two has now begun to grow.

32d day. The last tube has suddenly begun to grow.

No difference between the three and the control tubes noted; test-cultures gave a pure growth of staphylococcus pyogenes aureus.

Experiments similar to *A* and *B* were made with iodol and salol; all gave prompt results. Later, two tubes of each class were inoculated with staphylococcus pyogenes citreus and albus, staphylococcus citreus flavus and albus, and bacillus pyogenes fœtidus, making twenty tubes in all. All gave prompt positive results.

C. Three of the tubes from *B*, after the gelatine was all dissolved, were left in a window looking to the south for six weeks. At the end of this time test-tubes were inoculated from them. All gave pure growths of staphylococcus pyogenes aureus. When the cultures were made I also tested for iodine with starch paper, but failed to get any reaction.

D. Five tubes of solidified blood serum were inoculated with staphylococcus pyogenes aureus, and then covered with iodoform. Examination on the tenth day gave a good growth of staphylococcus pyogenes aureus in all.

E. Fifteen tubes of fluid blood serum were divided into sets of five, and had added five per cent. of iodoform, iodol, and salol, respectively; after this all were inoculated with staphylococcus pyogenes aureus.

8th day. Iodoform tubes almost clear, with a few flocculi; salol and iodol tubes a little muddy. Gelatine tubes, inoculated from the serum tubes, showed one of the iodoform and iodol tubes to be contaminated by a small white coccus, and the staphylococcus pyogenes aureus to be active in all. Starch-test for iodine was negative, though all the tubes had stood in the light.

F. A mixture of my experiment bacillus and iodoform, in the proportion of about one to thirty, was made, and allowed to stand for twenty-four hours; the mixture was then planted on gelatine, with a prompt positive result.

Besides the above, I made some two hundred experiments with all sorts of bacteria, gelatine, and five per cent. of iodoform; all gave good growths, though some were retarded, as in the two tubes mentioned in *B.* This retardation I attribute to the mechanical separation of the bacterium from the gelatine. The bacteria could easily be separated from the gelatine when shaking the tubes to distribute the iodoform over the surface, when, thanks to the repellant property of iodoform to water, they would have no access to food for growth. That the bacteria were not killed is shown by their ultimately taking.

As it has been claimed that iodoform only acts in solution, as in iodoform oil, either directly or indirectly, iodine being set free, the following set of experiments was made with milk. Milk was selected on account of its containing fats, albuminoids, sugar, and salts, thus offering opportunity for dissolving the iodoform, and for the formation of derivative products, which might break up the iodoform into active principles.

G. To each of four tubes of sterilized milk, containing five per cent. of iodoform, was added a drop of staphylococcus pyogenes aureus in water. In all the following experiments the bacterium used was suspended in sterile water, and one drop of the water used for each tube, by this means a uniform inoculation was procured.

7th day. Little apparent change, agar-agar tube inoculated from each.

13th day. All the agar-agar tubes contain pure cultures of staphylococcus pyogenes aureus in good development.

H. Two tubes of milk, handled the same way as *G*, with the omission of the iodoform, gave the same results.

I. Five tubes of sterile milk with five per cent. of iodoform were inoculated with water, containing my experiment bacillus.

5th day. Cream normal, below the cream a clearish zone, like soapy water, of fluid, lower three-fourths of milk appears normal; no visible curds.

10th day. Same, except clear zone much larger. Agar-agar tubes inoculated.

14th day. Three of the agar-agar tubes pure, two impure. Milk gives no iodine reaction with starch paper.

J. As a control to *I*, five other tubes of milk, without the iodoform, were put through the same routine. They finally gave pure cultures of the bacillus. At no time did they differ in appearance from *I*.

K. Five tubes of sterile milk, with five per cent. of iodoform, were inoculated with a drop each of water, containing bacillus butyricus.

2d day. No visible change.

4th day. No visible change.

8th day. No visible change; no smell; but a very disagreeable, bitter taste in all. Agar-agar tubes inoculated.

15th day. Three of the agar-agar tubes contain pure cultures, two impure, of b. butyricus.

L. Five other tubes of sterilized milk, without iodoform, put through the same course, gave the same results.

M. A short, stout bacterium was isolated from some very foul milk, and used in the fifth generation in the following experiment. It was selected on account of the very active changes it caused in milk. Five tubes of sterilized milk, with five per cent. of iodoform, were inoculated by a drop each of water with the bacillus.

3d day. Cream softened up, more fluid, looks oily; milk clearer at the top, bottom denser; decided putrid odor. Agar-agar cultures made.

6th day. Agar-agar cultures pure.

N. This series was the counterpart of *M*, with the exception of the iodoform. No difference, except more smell, could be detected.

O. In the following was used what I presumed to be the putrefying, fluorescing bacillus, and five tubes of five per cent. of iodoform milk.

3d day. All coagulated in a big clot, whey clear.

10th day. Bad stink, like cheese. Agar-agar cultures made.

14th day. Four agar-agar cultures, pure and impure.

P. Control to *O.* No difference observed in the milk or the stench. Agar-agar cultures, all pure.

All the above experiments gave negative results, except the more moderate stench in the tubes inoculated with putrefying bacteria, and confirm those of Lübbert, Heyn, and Rovsing. They show that iodoform is not a germicide, and since cultures were got from all, are opposed to Sänger's result with the anthrax bacillus.

Next I took up the thread experiments of Sattler, and shortly satisfied myself that they were accurate. Growth does not readily take place from contaminated threads, provided they have been dusted with iodoform. But this in itself is no proof of any action other than physical on the part of the iodoform. The bacteria must be brought in contact with the nutritive material in order to grow, and by covering the threads with iodoform we do our best to separate them from it. Ordinarily, the fluid absorbed would be sufficient to start their growth, did not iodoform, like lycopodium, repel water, and thus keep the threads dry. To test the above, ninety threads contaminated with staphylococcus pyogenes aureus, were divided into lots of thirty and dusted with iodoform, chalk, and lycopodium; all were then carefully laid on gelatine plates. The average taking of the threads was as follows: chalk, second day; lycopodium, fourth day; iodoform, fifth day. In view of the action of the lycopodium, it does not seem safe to draw any conclusions from this kind of experiments.

Having noted as Mikulicz points out, that fluids to which iodoform had been added remained clear longer than usual, the following set of experiments was made with bouillon:

Q. To three tubes each containing ten cubic centimetres of bouillon was added five per cent. of iodoform, and then one drop of water with staphylococcus pyogenes aureus. Three more tubes exactly the same, except for the iodoform, were prepared and all six stood in the same rack. At the end of twenty-three hours agar-agar plate cultures, each containing one drop of bouillon, were made. The plates made from the tubes with iodoform came very thick, too thick to count, yet distinct; those made from the simple bouillon were just like ground glass.

The above was repeated three times, but the drop of bouillon was diluted in ten cubic centimetres of water, a drop of which was taken for the plates. These could be counted and gave from two to four times as many colonies in the plates made from the simple bouillon as in those with iodoform. For instance, one lot gave me 20,000, 20,000, and 21,000, as compared with 5000, 6000, and 7000. Of course, the counting, or reckoning of so large a number of colonies is not accurate, but does not give room for any such errors as the difference mentioned above.

R. The same as *Q*, except that the fluorescing putrefying bacillus was used. Here the count gave 10,600, 10,000, and 14,300, against 33,000, 31,000, and 34,000 without iodoform. Besides this, at the end of ten

days the iodoform tubes had but a moderate odor, while the others were very offensive.

Here at last we have proof, from laboratory experiments, of a detrimental action of iodoform on bacteria; for, by counting, the difference is clearly brought out. The greater or less degree of smell is of doubtful significance, since iodoform rapidly allays the stench from putrid bouillon. Here, since the bacteria have already produced the foul odors, the action cannot be due to diminished propagation, but a direct chemical action of the iodoform on the noxious substances is probable, unless its odor hides them.

The bacterial growth of ulcers and wounds is not capable of being reduced to a count, but by making cultures before and after the use of odoform, some idea can be got. For this purpose I selected six ulcers of the leg, because there was little chance of the iodoform doing any good; if the wounds had improved, any difference in bacteria might be justly attributed to the improvement and not to the iodoform. First, the lightest possible line culture was made, then the ulcers powdered with iodoform, and at the end of two days a new line culture made. Comparison of the cultures made before and after the use of iodoform showed a much stronger growth in the first set. This difference is to be ascribed in part to the direct action of the iodoform and partly to the drying of the wound. For iodoform does check the secretion and thus produce a food famine among the germs.

As a result of all the experiments, we may safely conclude that iodoform has no direct action as a germicide, a result agreeing with Heyn and Rovsing, also, that germicidal products are, probably, not derived from it in the presence of growing bacteria. On the other hand, the experiments with bouillon show that the presence of iodoform markedly retards the growth of bacteria, an action not before proven, and diminishes the foul odors of putrefaction. The diminution in odor cannot be ascribed to the action of iodoform on the bacteria, as pointed out before.

Why the iodoform failed to give positive results in the Heyn and Rovsing class of experiments is obscure. It may be due to the fact that in spite of the iodoform, crowding quickly occurs, the injurious effects of which, by restricting, quickly obliterate the action of iodoform.

Looked at from the clinical side, the ultimate object of all medical research, the following rules may be accepted:

1. Iodoform not being a germicide is not a fit substance to use to procure asepsis of instruments, materials, or wounds.

2. Iodoform is allowable, with the present state of our pharmacopœia, in infected wounds where the true germicides are contraindicated, as by danger of poisoning or impracticability.

3. As has long been known, iodoform has a decided tendency to stop serous oozing, and, therefore, may be indicated in wounds where the moisture threatens the integrity of the aseptic or antiseptic dressing.

[Reprinted from the Boston Medical and Surgical Journal of October 25, 1888.]

VALVES IN THE VEINS OF THE HUMAN INTESTINES.[1]

BY W. S. BRYANT, A.M., M.D.

HISTORY.

CHARLES ETIENNE described valves in the portal system of man, which he called apophyses, and which he compared to the valves of the heart. Bauer met valves in the short veins which run from the stomach to the splenic veins in man, which Cruveilhier was unable to verify.

Milne Edwards says that among mammals there are no valves in the portal system, but that in some species one meets in these vessels valves more or less numerous.

" Dr. Crisp, of England, has described valves in the splenic veins in some of the inferior animals. In one of the mesenteric veins of the reindeer he showed forty-two pairs of valves." (From the *N. Y. Med. Journal*, 1865.)

Hyrtl, in the *Lehrbuch der Anatomie*, says : " Only in the portal vein of the rodents have I found a very pretty spiral valve of three to eight revolutions."

Sappey has described some valves belonging to the portal system of veins. These were found in the falciform ligament of the liver in very small veins.

Dr. Hochstetter recently, in the *Archiv für Anatomie und Physiologie* (1887), from whose article I take the liberty to quote at some length, because his is the most complete work on the subject, notes the following observations, which I have been able to confirm for the most part. He found that in the newborn infant the venous branches upon the stomach were furnished with valves, one in each minor branch, very rarely two. These valves were bicuspid, and usually placed in those vessels at or near their openings into the main branches. The branches of the coronary vein of the stomach oftentimes appeared to

[1] A Graduation Thesis. The investigations were made in the Anatomical Department of the Harvard Medical School in the winter of 1887-8.

have a relatively small number of valves. In the great omentum the veins had valves at the venæ gastro-epiploicæ, and wherever smaller branches emptied into greater. "With increasing age, the valves become little by little incompetent: first, in the gastric branches of the vena gastro-epiploica dextra near the pylorus, next along the great curvature in the branches of the vena gastro-epiploica sinistra and in the venæ gastricæ breves, so that they persist longest in the branches which empty into the communication (if there is any) between the two venæ gastro-epiploicæ. Towards the twentieth year of life no venous branches along the great curvature of the stomach seem any longer to possess competent valves. Comparatively late the valves of the superior branches of the coronary vein become incompetent, and, last of all, usually those in the veins of the great omentum, where I sometimes found, even in very old people, perfectly competent valves."

"The valves become incompetent with increasing years by becoming smaller little by little, so that the two opposite edges of the valves no longer touch when the vessel is completely filled. Finally, only two bow-shaped linear seams, corresponding to where the valves had been attached, remain, and these can disappear, so that no trace of the valves is left." Dr. Hochstetter goes on to state that in many animals valves are found in the gastric veins, and in some they are more numerous than in man.[2] In the horse, he found them throughout the gastro-splenic veins. He also found them in the main, as well as the small branches of the gastric veins in antelope dorkas, sheep, goat, and in carnivora (dog, fox, cat, and otter). They were most numerous and best developed in the dog and cat. He found valves also as well developed and as numerous in the horse and hog as in the beasts of prey. In the rabbit he found only a very few valves, and these were in the gastric veins; while in the hedge-hog and bat (vesperugo noctula) he found none. On a close examination of two monkeys, he found good evidence of valves in the smaller veins, especially towards the great omentum and here and there in the branches of the gastric tributaries of the venæ gastro-epiploicæ, or there were dilatations, indicating the former presence of valves. Dr. Hochstetter says it is not surprising that the valves in the branches of the portal vein become incompetent with age when we consider that

[2] Henle had already mentioned valves in the branches of the portal vein of mammals. Weigle found them in ruminants and the horse. G. Leising, C. Müller, and F. Müller mention some valves in the splenic vein of the domestic animals.

many of the valves of the extremities do not last to maturity.

METHODS.

Professor Dwight has aided me with his able supervision in making the following observations:

Last winter, while making general injections of the abdominal veins in dogs, I noticed that the injecting mass rarely entered the small veins of the intestines, and, on close examination, found the obstruction was due to valves. I then made injections of the portal system in several animals and in the infant at birth, and lastly in adult man, with what results will be shown later.

The methods I used were calculated to show only competent valves. A coarse injecting mass that would not pass through the capillaries, such as gelatine thickened with Prussian blue, was used. If competent valves occurred, their presence was at once shown by the arresting of the mass in the course of the vein. When the mass is thus brought to a standstill, a minute examination is necessary to show the nature of the obstruction, and a microscope of low power is useful.

A number of the valves discovered in this way, with the vessel still distended, were hardened and sectioned with a microtome. The valves were then seen cut at various angles according to the plane of the section.

Figure 1 is from a microphotograph by Mr. G. W. Fitz, of a vessel from the jejunum of a woman, prepared and cut in the preceding way. The section is longitudinal to the vessel dividing the two cusps of a valve.

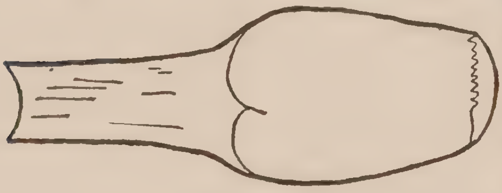


FIG. 1.

For a thorough view of the structure of the valves in contradistinction to their position, clear gelatine can be used for an injecting mass. The advantage of this is its transparency, for when a vein having a valve distended with clear gelatine is cleanly dissected, the outline of both its parts can be seen through the walls of the vessel, either with the naked eye, if the valves are large, or with a low power of the microscope if they are in the smaller veins.

OBSERVATIONS.— ANIMALS.

In the search for valves the veins of the intestines in the following animals were injected — guinea pig,

rabbit, cat and dog. No evidence of valves was found in the guinea pig. In only one rabbit the veins of the last four or five inches of the large intestine were found well furnished with valves. These valves were placed along the attached border of the intestine in the mouths of the venæ breves. Figure 2 shows a portion of that intestine (rabbit), natural size, with the valves diagramatically represented.

FIG. 2.

The cat was found to have a considerable number of competent valves at varying intervals along the intestine. They were usually most numerous in the vicinity of the cæcum, above and below, and occurred in a double series, one at the mouths of the venæ breves, and the other at the mouths of their principal tributaries.

The dog was found to be furnished with a more efficient set of valves than any of the other animals examined. The valves were found the whole length of the intestine with only here and there a spot where the injecting mass had entered the fine veins. In these places there was usually some indications of the presence of an incompetent valve. The valves usually occurred in a double series, more rarely triple or single. The most constant valves were at the mouths of the main tributaries of the venæ breves. The next most constant were at the mouths of the venæ breves, and the least constant were placed somewhere in the secondary tributaries. In one dog a valve was found in the inferior mesenteric vein a short distance from its lower extremity.

FIG. 3.

Figure 3 represents a portion of a dog's small intestine, actual size, with diagramatic valves.

MAN.

Infants at birth, children, and adults were examined.

INFANTS AT BIRTH.

In infants, valves are found at irregular intervals along the whole course of the intestines, more numerous on the large intestines than on the small. On the large intestine they occurred in the intestinal veins (venæ breves) at or near their mouths, and sometimes in the mouths of the main tributaries of these veins. This was more frequently the case on the cæcum. The number of valves varied with the individual; in the majority of cases more than half of the venæ breves of the large intestine contained valves.

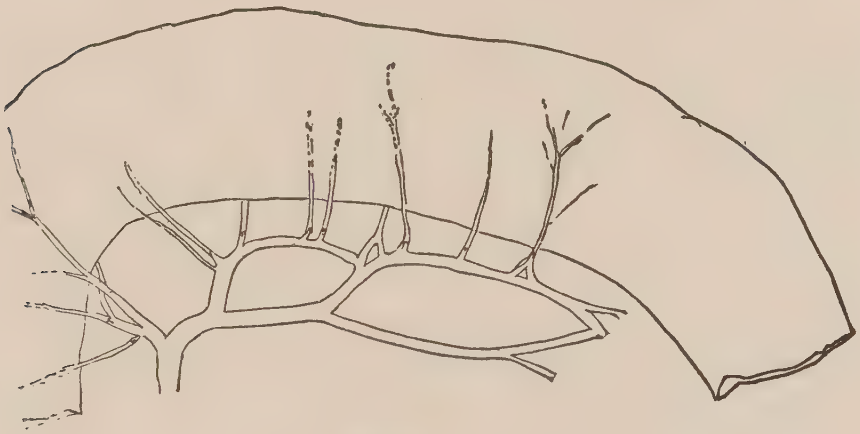


FIG. 4.

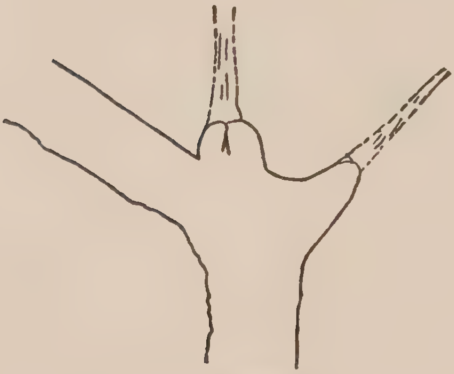


FIG. 5.

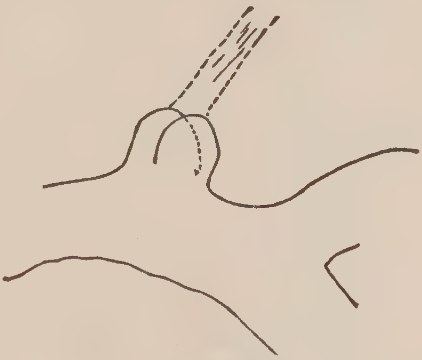


FIG. 6.

Figure 4 is a portion of the large intestine of an infant at birth, one and one-half natural size with diagramatic valves.

Figures 5 and 6 are from microphotographs by Mr. G. W. Fitz, of the valves themselves of the large intestine, injected with clear gelatine, showing the outline of both cusps of a valve, through the anterior wall of the vein. On the small intestine the venous valves

were arranged in somewhat the same way. They were found in the intestinal veins (venæ breves) at or near their mouths, and at the mouths of the superficial branches of these veins. There is a much smaller number of valves here than in the large intestine; however, they are rarely entirely absent and occasionally are found in as many as one in five of the venæ breves.

FIG. 7.

Figure 7 is the portion of the small intestine of an infant at birth, twice the natural size, and with diagramatic valves.

The following table shows the percentage of valves found in a number of the infants examined, taking 100 as the greatest possible number; that is, one at the mouth of each one of the venæ breves:

No. 1.	In large	intestine,	100%...	In small	intestine,	20%
2.	"	"	100 ...	"	"	$\frac{3}{5}$
3.	"	"	90 ...	"	"	$\frac{1}{2}$
4.	"	"	87 ...	"	"	$1\frac{1}{5}$
5.	"	"	85 ...	"	"	12
6.	"	"	84 ...	"	"	4
7.	"	"	80 ...	"	"	$\frac{1}{5}$
8.	"	"	77 ...	"	"	$\frac{1}{10}$
9.	"	"	63 ...	"	"	$\frac{17}{100}$
10.	"	"	32 ...	"	"	1
11.	"	"	25 ...	"	"	10
12.	"	"	20 ...	"	"	0
13.	"	"	10 ...	"	"	$\frac{1}{2}$
14.	"	"	4 ...	"	"	$\frac{1}{10}$
15.	"	"	2 ...	"	"	0
16.	"	"	0 ...	"	"	$\frac{1}{20}$
17.	"	"	0 ...	"	"	0

CHILDREN.

Only three children were examined. The youngest was about two weeks old. The large intestine had valves in about ten per cent. of the venæ breves, and in the small intestine three valves only were seen. The next child was about five months old. In this there were only four valves found which were on the jejunum. The third child was ten years old and quite rachitic. No valves were found on the large intestine. The valves found on the small intestine were most numerous in the jejunum. There were twelve valves in the venæ breves, and thirty valves in as many superficial branches of the venæ breves. These superficial tributaries arise from the surface of the intestine and empty into the venæ breves a short distance from the intestine.

ADULTS.

A number of human adults were examined with the following results. Few valves were found in any one individual, even the youngest, and it did not appear that age after adolescence had much influence on the number of valves on the intestines. When the veins in the other parts of the great omentum were intact, I noticed that valves were less numerous along the free border in those veins which in the erect position would be nearly horizontal. Also that the disappearance of valves seemed to begin in one or two of the main veins lying near the centre of the omentum, and that the valves disappeared in the direct course of these veins before they did in their tributaries.

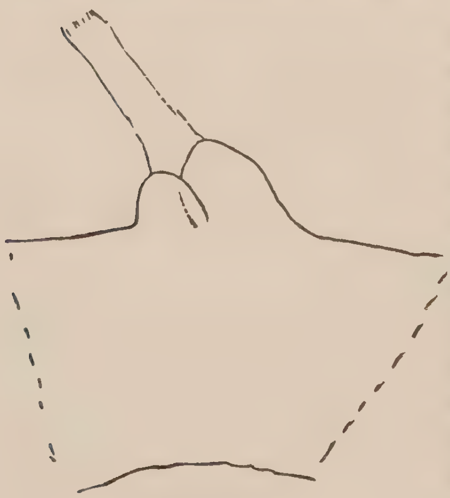


Fig. 8.

In the intestines of most adults there were a few valves remaining in the venæ brevis. These were usually on the ileum near the cæcum.

Figure 8 is from a microphotograph by Mr. G. W. Fitz of a valve from the cæcum of an adult. The injecting mass is clear gelatine, and the outlines of both the cusps of the valve are seen clearly through the wall of the vessel. Valves in the superficial branches of the venæ breves occur very frequently on

the jejunum. They are most numerous at the mouths of these veins, but there are sometimes two more sets of valves in other portions of the superficial vessel, making in all three sets of valves.

FIG. 9.

Figure 9 is a fragment of the small intestine of an adult man considerably enlarged, showing superficial branch of vena brevis, containing diagramatic valves.

As autopsies were the chief source of material, in but few cases the whole intestine could be examined, and in many only a small portion, so that the table is not as complete as might be desired.

The table on the following page shows the number of competent valves found in the various parts of the intestines of human adults.

The valves of the intestinal veins, like those of the rest of the portal system, are bicuspid. They are also thin and delicate, and in the infant extremely elastic. In man the line of contact of the two halves of the valves, in the majority of cases, is nearly parallel with the course of the intestine.

The number of subjects intermediate between the infants at birth and the adults which were examined is very small, and the conclusions based on them may have to be altered after a wider experience.

No.	On Cæcum.	On rest of Large Intestines.	On Small Intestines.		Total.
			In venæ brevis.	In superficial branches.	
1	5	2 on ascending colon.	22 (2 feet examined.)	56 (2 ft. examined).	85
2	Not exam.	6	3	43	52
3	1	11	18	20	50
4	1	None in ascend. col.	9 (mostly in ileum).	37 (mostly in jejun.).	47
5	1	3	7	23	34
6	None.	5 (also 4 non-comp.).	3 (mostly in ileum).	15 (mostly in jejun.).	23
7	"	6	13 " "	None.	19
8	"	None.	1	12	13
9	"	1	3	7	11
10	5	None in ascend. col.	Not examined.	Not examined.	5
11	None.	" "	None.	3	3
12	"	None.	"	None.	
13	"	None.	"	"	
14	"	None.	"	"	
15	Not exam.	Not examined.	"	"	
16	"	" "	"	"	
17	"	None in transver. col.	Not examined.	Not examined.	
18	"	" "	None in jejunum.	None in jejunum.	
19	"	" "	Not examined.	Not examined.	
20	"	Not examined.	None (2 ft. exam.).	None (2 feet exam.).	

CONCLUSIONS.

These observations show that at birth the valves on the intestines are quite numerous in man, and at this age they are more abundant on the large intestine. Also that in a few months the valves either disappear or become incompetent, with few exceptions. In adult man there are usually a few valves, and these are more abundant in the small intestine, especially in the superficial tributaries of the venæ breves. These valves are more numerous in the jejunum, and disappear as we approach the cæcum.

Though the valves just described were discovered since Hochstetter's paper was written, his remarks on the significance of valves in the portal system will apply to them. After alluding to the fact that the presence of valves in the branches of the portal vein seems widely spread among mammals, he says: "But only in individual species, as in the beasts of prey, do the valves possess great importance in connection with the circulation. In many species they are to be considered much more as rudimentary organs, which act either only in youth, as in men and perhaps the ape, and later in part or wholly disappear, or for the most part, as in the rabbit, are very imperfect."

Reprinted from the New York Medical Journal for November 3, 1888.

EFFECTS OF VARYING RATES OF STIMULATION ON THE ACTION OF THE RECURRENT LARYNGEAL NERVES.*

FROM THE PHYSIOLOGICAL LABORATORY OF THE HARVARD MEDICAL SCHOOL.

By FRANKLIN H. HOOPER, M. D.,
BOSTON.

In a preliminary communication on this subject† Professor Henry P. Bowditch and I recorded the fact that the rate of stimulation of the recurrent laryngeal nerves was an important factor in producing an opening or a closing of the glottis. We stated that slow rates of stimulation (18 to 28 induction shocks in a second) with weak currents produced in dogs an opening of the glottis, while with more rapid rates (30 to 40 a second), the same feeble current being used, closing was the result. We also surmised that the reason that other observers had obtained an opening of the glottis with weak currents might be sought in the slow rate of vibration of the interrupters they had employed. We submit the following experiments to substantiate the statements set forth in our former communication. These experiments have been performed in conjunction with Professor Bowditch, and witnessed by many others besides ourselves. Eleven dogs and four cats have been devoted to the investigation. One dog out of this number, under the influence of chloral, failed to exhibit the phenomenon so constant in all the other animals. In this exceptional dog we obtained only closings of the glottis with all rates and all intensities applied to the recurrent nerves. We have recorded in previous papers‡ that in dogs sulphuric ether

* Read before the American Laryngological Association at its tenth annual congress.

† "New York Medical Journal," November 26, 1887.

‡ "The Respiratory Function of the Human Larynx," "New York Medical Journal," July 4, 1885. "The Anatomy and Physiology of the Recurrent Laryngeal Nerves," "New York Medical Journal," July 9, 16, 23, and August 6, 1887.

reverses the normal action of the recurrent nerves; the glottis, under the influence of this drug, dilates instead of closing on stimulation of the recurrents. In order, therefore, not to be embarrassed by the "ether effect" in our present research upon the effect of varying rates of stimulation, we used chloralized dogs, inasmuch as we had never been able to produce by the methods previously employed a dilatation of the glottis in dogs under the influence of this narcotic. We propose now to describe in detail the apparatus and methods employed in this investigation, and to give a few tables of typical experiments.

The interrupter used was constructed by Professor Bowditch on the principle of Bernstein's acoustic current-breaker. Its construction is shown in Fig. 1. It consists of a vibrating-rod, *A*, the length of which is readily altered by pushing it between steel rollers, *B*, which grasp it firmly. The pressure of the rollers upon the spring is regulated by the screw *C*. The spring, mercury-cup, and electro-

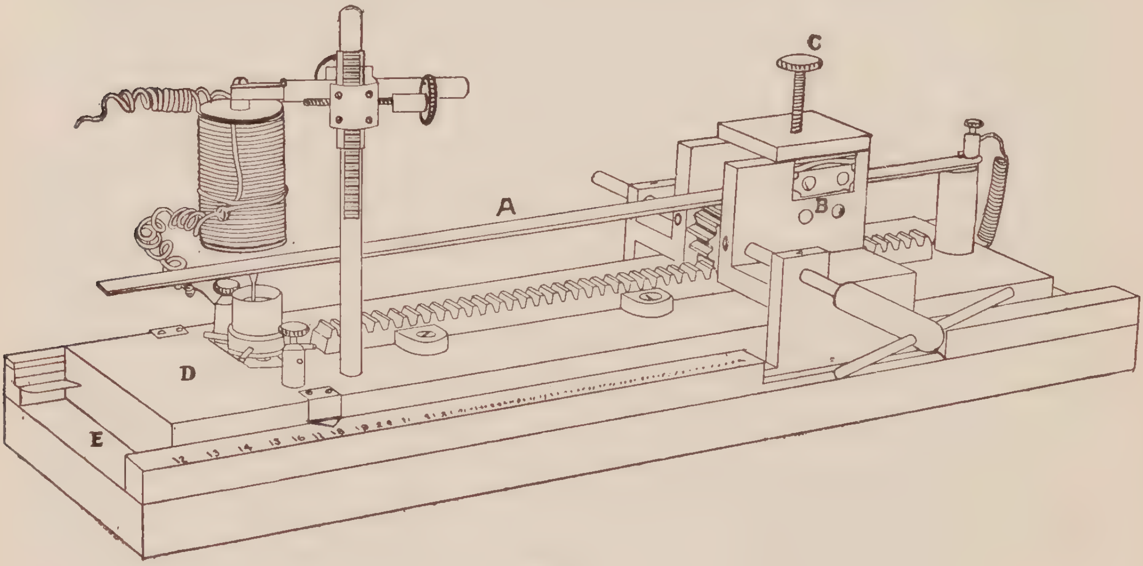


Fig. 1.

magnet are supported upon a board, *D*, which is made to slide by a rack and pinion movement upon a second board, *E*, which supports the rollers. A scale graduated empirically upon the board, *E*, indicates, by the position of a pointer, the number of vibrations from 12 to 80 made by the spring in different positions of the apparatus. A second form of interrupter was also employed, which was constructed by Mr. G. W. Fitz, who furnishes the following description of it:

"The interrupter shown in Fig. 2 consists essentially of a steel vibrator (*A*), on which a weight (*B*) slips to regulate the time of vibration. To the weight is attached a cord, which passes over the

end of the vibrator and around the index dial (*C*) and setting head (*D*). By turning *D*, the weight might instantly be changed to any point on the vibrator without stopping its motion—the rate being read on the index dial (*C*). The core of the magnet (*E*) is hollow, and the soft iron process from the vibrator plays within, rendering adjustment unnecessary."

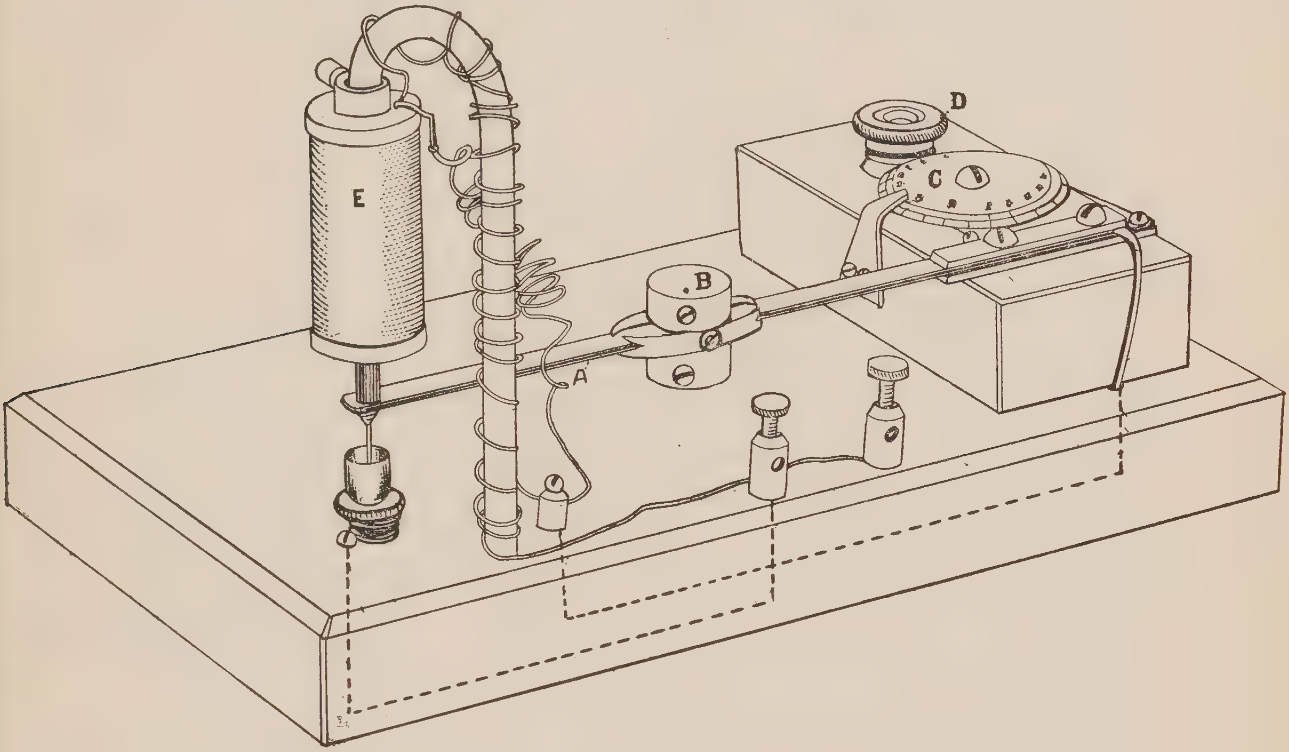


FIG. 2.

The animals were arranged for observation in the manner hitherto described by us in previous papers. Shielded electrodes were placed on both recurrent laryngeal nerves, which were connected with an induction coil, the latter being in communication with the interrupter. By this arrangement the nerves could be stimulated at will with any intensity of current * and with any rate of vibration.

In explanation of the letters used in the following tables, we may say that O indicates an opening of the glottis; OC, an opening in the posterior portion of the glottis, and a contraction of the ligamentous portion in front, which gives to the glottis a rhomboidal shape—the appearance, in fact, to which we have alluded in previous communications as the "mixed movement"; C signifies complete closure of the glottis; V, vibrations of the vocal bands.

* The intensity of the current is given in arbitrary units, calculated by a method described in a previous paper. "Experimental Researches on the Tension of the Vocal Bands," "Trans. of the Amer. Laryngological Assoc.," 1883, p. 121.

November 3, 1887.—Mongrel fox terrier dog; medium-sized; eleven months of age; chloralized.

TABLE I.

	INTENSITIES.	RATES OF VIBRATION.					
		20	24	28	32	36	40
a	2	O	O	O	OC	OC	C
b	3	O	O	OC	OC	C	
c	4	O	O	OC	OC	C	
d	5	O	O	OC	C		
e	6	O	O	OC	C		
f	7	O	OC	C			
g	8	OC	OC	C			
h	10	C	C				

It will be seen by the perpendicular column of figures in the above table that the intensity of the stimulation varied from 2 to 10. The horizontal line of figures, indicating the rates of vibration, varied from 20 to 40 in a second. Starting at *a*, with the very feeble irritation of 2, it will be observed that with the rates 20, 24, and 28 opening of the glottis was produced. On increasing the rates to 32 and 36, a contraction of the glottis in front was noticed, the opening behind persisting (mixed movement). With still more rapid rates (40) complete closure took place. In this first trial, then, with a very weak irritation, opening was effected with rates of vibration as rapid as 28; after this there was a tendency to close, and with a rate of 40 complete closure supervened. On increasing the intensity to 3 (*b*) and using the same successive rates of vibration, it will be seen that the tendency to close began with a rate of 28 and complete closure occurred at 36. The third trial (*c*) with an intensity of 4, gave the same results as when an intensity of 3 (*b*) was used. At the fourth trial (*d*), intensity 5, openings took place as with previous rates and intensities, but closing was effected with a rate of 32. It will be observed, therefore, that as the intensity of irritation was increased, closing took place with slower rates of vibration; and finally (*h*), with an intensity of 10, closing was effected with a rate of 20, which at the first trial (*a*), when the intensity was 2, gave complete opening, and in which it was necessary to increase the rates to 40 before closing could be elicited.

November 10, 1887.—Scotch terrier dog; old, blind, and feeble; chloralized.

An analysis of the following table shows that the results do not differ in any marked degree from those of Table I. We note in

Table II the vibration of the vocal bands with the weak intensity of 2, and a slow rate of vibration. This vibration of the vocal bands was synchronous with the rate of interruption and was very interesting to watch. On increasing the rate of interruption an opening of the vocal bands was combined with their vibration, yet when a still more rapid rate (22) was used, a pronounced opening was obtained, all vibratory movements of the vocal bands having disappeared.

TABLE II.

Intensities.	RATES OF VIBRATION.											
	18	20	22	24	26	28	30	32	34	36	38	40
2	V	VO	O	O	O	O	OC	OC	OC	OC	OC	C
3	O	O	O	O	O	OC	OC	OC	C			
4	O	O	OC	OC	OC	OC	C					
5	O	O	OC	OC	OC	C						
6	O	O	OC	OC	OC	C						
7	O	O	OC	OC	OC	C						
8	OC	OC	OC	OC	C							
10	OC	OC	OC	C								
12	C											

May 9, 1888.—Spaniel dog; medium sized; one year of age; chloralized.

TABLE III.

INTENSITIES.	RATES OF VIBRATION.								
	18	20	22	24	26	28	30	34	40
1	V	VO	VO	VO	VO	OC	OC	C	C
2	VO	VO	O	O	O	OC	C	C	
3	VO	O	O	OC	OC	C			
4	O	O	OC	OC	OC	C			
5	O	OC	OC	OC	C				
6	OC	OC	OC	OC	C				
10	OC	OC	OC	C					
15	OC	C	C						
30	OC	C							

The point of the effect of varying rates is well shown in the foregoing table. A very convincing manner of demonstrating the phenomenon is by using two interrupters, the connections with the induction coil being so arranged that by turning a switch the current can be directed through either of the interrupters. In this way stimulations, consisting of induction shocks of any two desired rates, can be alternately sent through the nerve in rapid succession. In the animal which was the subject of the experiments on May 9th, it

was determined that with an intensity of 3, rate 40, a very sharp closure of the glottis was produced, while with the same intensity and a rate of 20 an opening of the glottis was effected. Now, by arranging the two interrupters so that the one vibrated 40 times in a second and the other 20, by simply shifting the switch either of these rates could in an instant be passed through the nerves, and an opening or a closing of the glottis followed according to the rate employed.

Table IV.

INTENSITY.	Rate of first interrupter. 20	Rate of second interrupter. 40
3	O	C

The above-described experiments were performed, as already stated, on chloralized dogs, and seemed to us to be very conclusive. They show that starting with the slowest rate and the feeblest intensity that could produce any noticeable effect, an opening of the glottis was the phenomenon constantly observed. This opening could be converted into a closing by increasing either the rate or the intensity. But the closing was produced much more readily by increasing the rate than by increasing the intensity. The tables show that doubling the rate was sufficient to call forth a distinct closure, while it was necessary to increase the intensity from three to four fold. It is evident, therefore, that the cause of the varying results of nerve stimulation with different rates can not be sought in the amounts of electricity which were thus applied to the nerves.

In previous communications we have fully detailed the "ether-effect" phenomena, and have recorded that sulphuric ether reverses the normal action of the recurrent nerves; the glottis, under the influence of this drug, dilates instead of closing on stimulation of the recurrents; that the "ether-effect" varies according to the susceptibility of the dog, to the amount consumed, and to the intensity of the irritation. Under small doses of ether, stimulation produces closure of the glottis; under large doses, complete dilatation.

The following experiments (Table V) show very clearly both the "ether-effect" and the effects of varying rates of stimulation.

January 19, 1888.—Black-and-tan bitch; about one year of age; tracheotomized. Ether administered through tracheal cannula.*

* See cut, "New York Med. Journal," July 23, 1887, p. 100.

TABLE V.

INTENSITIES.	RATES OF VIBRATION.					
	18	28	34	40	50	80
0·1	O	O	O	O	O	O
2	O	O	O	O	O	O
3	O	O	O	O	O	O
6	O	O	O	O	O	O
10	O	O	O	O	O	O
15	O	O	O	O	O	O
20	O	O	O	O	O	O
30	O	O	O	O	O	O

It will be seen by the foregoing table that when the animal was profoundly etherized, an opening of the glottis was effected with all intensities and with all rates.

The ether was now removed, and, as its effects were passing off, the nerves were stimulated from time to time with different intensities and with varying rates. The results are given in the following table:

TABLE VI.

HOUR.	Intensities.	RATES OF VIBRATION.			
		18	28	34	40
10.46	1 to 30	O	O	O	O
10.48	15	OC	OC	C	
10.49	2	O	OC	C	
10.50	2	O	OC	C	
10.50	30	C			
10.52	15	C			
10.54	2	O			
10.54	13	C			

In our paper last year, already referred to, we stated that our experiments on cats showed that the normal action of the recurrent nerves in that animal was to produce an opening of the glottis instead of a closure, as in the dog. We must now modify that statement, as we have proved that with rapid rates of vibration a closure of the glottis may be called forth in cats. It requires, however, nearly double the rate in the cat that is necessary in the dog. As far as our investigations go, they teach us that, as a rule, in the dog, a rate of 40 vibrations in a second is followed by complete closure of the glottis, while in the cat it requires from 70 to 80.

February 16, 1888.—Female cat; chloralized.

TABLE VII.

INTENSITIES.	RATES OF VIBRATION.					
	15	20	30	50	70	80
1	O	O	O	O	OC	C
3	O	O	O	O	OC	C
10	O	O	O	OC	OC	
20	O	O	OC	OC	C	
50	O	OC	OC	C		

This table may be taken for the type of our experiments on cats. Experiments with the double interrupters were also made in the same manner as described for the dogs. One of the interrupters, being set to vibrate at 80, with a feeble current, gave complete closure, while the other, vibrating at 25, produced opening.

TABLE VIII.

INTENSITY.	Rate of first interrupter. 25	Rate of second interrupter. 80
2	O	C

We may summarize our conclusions as follows:

1. It requires a more rapid rate of vibration to produce a closure of the glottis in cats than in dogs.

2. In cats the rate of stimulation necessary to effect a closure must be from 70 to 80 a second; in dogs from 30 to 40.

3. Rates slower than 70 a second produce in cats an opening of the glottis; in dogs, rates slower than 30 produce opening.

4. The intensity of the current influences the effect of varying rates of stimulation.

5. Weak currents with slow rates produce opening.

6. By increasing the rates, the intensity remaining the same, closing results.

7. By increasing the intensity, the rate remaining the same, closing results.

8. Closing is produced more readily by increasing the rate than by increasing the intensity.

Extracted from the
American Journal of the Medical Sciences for May, 1888.

ON THE

STERILIZATION OF MILK AND FOODS FOR INFANTS.[1]

By J. Amory Jeffries, M.D.,
of Boston.

During last summer's work as District Physician of the Boston Dispensary, my attention was called, by the severity and prevalence of the trouble, to the summer diarrhœas of infants. The disease was quite prevalent, and in my cases severe, as the milder ones were all treated as out-patients. In most cases it had existed for about a week, and the infant was already collapsed by the time of my first visit. The skin would be found cold, the limbs were blue, the fontanelles sunken, eyes the same, the child either indifferent or curled up and whining. The pulse would be weak, small, and rapid, and the temperature, in the rectum, over 100° F., often higher; convulsions were rare. The bowels were tender and in most of the cases diarrhœa existed; a few, however, were constipated. The tendency of all discharges was to a green color and much mucus. In short, the clinical picture was that with which all physicians are familiar.

Treatment was begun with the ordinary drugs and salicylate of soda, which was then being so much extolled as a germicide, and care of the bottle. Later the treatment was changed to creasote, if vomiting existed, and care of the milk supply. The change was made to bring the treatment more into accord with the generally accepted belief that bacteria are at the bottom of the trouble. Whether special organisms are accepted as probable or not, the belief that changes wrought in the food, inside or outside the body, are the cause of the trouble is held by most.

Long before bacteria were thought of as the cause of the disease the usual routine treatment had a decidedly germicidal tendency. Gray powder probably acts as a mild germicide in the stomach, and lower down in the canal helps to sweep them out; while the care of the bottle

[1] The work for this paper was done in the bacteriological laboratory of the Harvard Medical School, for the freedom of which I am much indebted.

is nothing but an effort to avoid bacteria and their effects. Lastly, a host of patent foods, advertised as germ-free, have come into the market.

It is a curious fact that while all older people are chiefly fed on sterilized food—that is, cooked food—infants are fed on food peculiarly adapted, by its composition and fluid state, to offer a home to bacteria.

Investigation of the milk supply soon showed that this was greatly at fault; though "fresh each day," it was, as a rule, found to be decidedly acid, often curdling if heated, by the time it was fed to the infant. This the infant, unable to talk and knowing nothing better, was obliged to take or go empty. So, remembering the custom of housewives to scald the milk, directions were given that all milk used for the infants should at once, on receipt, be steamed in a skillet set into the top of the tea-kettle. After this it was kept covered and on ice if possible. The bottles were rarely clean, but were, as a rule, emptied after each feeding. The result was that instead of staying at the point of death, the little patients began to pick up and were soon well, the stools first becoming light, then yellow. I have since undertaken to devise some way by which milk can be practically sterilized—to lay down a rule applicable in any house, by any ordinary nurse.

The ordinary milk supply of a large city is a day or more old, has a slightly acid reaction, and contains many growing bacteria. If kept for a day it is decidedly acid in reaction, has a sour taste, is apt to curdle if heated, and contains a very large number of bacteria, the cause of the changes. Fresh milk sterilized, or collected sterile and protected from organisms, undergoes no changes even after the lapse of indefinite periods except the separation of the fats. If bacteria are present, a great variety of changes may occur according to the species—for instance, the milk sugar may be turned to acids, the fats broken up, or tyrotoxicon formed. As milk affords such a fine medium for growth, all efforts to rid it of bacteria must be governed by the use of poisons—germicides—or some physical condition inimical to their life. The first method is not admissible in foods, while the other offers little chance of success except by heat. Cold simply retards their growth, does not kill. As boiling produces marked changes, this also is undesirable, so our means are narrowed down to the ordinary one of steaming. Fortunately this produces but slight changes in the milk compared to boiling, and, as I have found, is efficient.

Before reporting the experiments a few preliminary statements are necessary. The milk used came from a private source, eight miles from Boston, was either eight or twenty-two hours old, was kept in an ice-chest, and of very good quality. The only precaution, in the way of sterilization, taken was to heat the test-tubes or flasks in an oven before using them. The flasks were also plugged with cotton-wool rather than a stopper, which is liable to be blown out.

The agar-agar tubes, mentioned in the experiments, are the regular ones used by bacteriologists—that is, test-tubes about one-fourth full of a meat-peptone solution, solidified by the addition of agar-agar. These were allowed to cool on an inclined plane to obtain a large surface. The loop is simply a small loop in the end of the platinum wire, used to convey the substance to be tested for bacteria to the surface of the agar-agar. Esmarchs are made by adding the substance to be tested to some meat-peptone gelatine solution, dissolved by warmth, and then rapidly rotating the test-tube containing it on a horizontal plane of ice. By this means the gelatine solidifies in a thin, even coat on the entire inner surface of the tube. Later the bacteria multiply and form colonies easily counted.

In making inoculations from the milk after steaming all precautions against contamination were taken; the fluid being first thoroughly mixed, so as to obtain due proportions of all the constituents in the part used for inoculation. After steaming the flasks were left at the temperature of the room, as ice, by virtue of its cold, would tend to diminish the severity of my tests.

All the experiments are reported, not only those which were favorable.

EXPERIMENT I.—Thirteen test-tubes of fresh morning's milk were placed in the steamer and heated until steamed for fifteen minutes. On the next day six of the above were steamed a second time the same way. Before the first steaming an agar-agar culture was made from the milk; in three days the growth was marked.

10th day. Agar-agar cultures made from the test-tubes of milk, one loop in each case.

13th. All the cultures sterile as yet.

26th. One-half of the cultures of the milk steamed but once, show growth; rest sterile.

EXPERIMENT II.—Agar-agar cultures made from the middle of a freshly opened can of Anglo-Swiss condensed milk. In ten days these developed three colonies. At the same time a 10 per cent. solution of the condensed milk was made with hot water, and steamed as in Experiment I., in all eighteen tubes.

2d day. Nine tubes steamed a second time.

9th. One of the tubes steamed but once has coagulated, all the others have a cream on top, a milky fluid below, and a slight sediment at the bottom. From each tube an agar-agar culture with one loop was made.

24th. Two-thirds of those steamed but once have coagulated, and their cultures show growth; the rest, same as before, and cultures sterile.

EXPERIMENT III.—A mixture was made of Mellin's food as directed on the bottle, of 8.5 grammes of the food and 131 c. cm. of water and milk each. This was then placed in eighteen sterile test-tubes, divided into two lots: (A) steamed once; (B) steamed twice, as in I.

10th day. Both A and B have a brownish cream, fluid, and sediment, but show no signs of coagulation. Agar-agar cultures made with one loop from each.

20th. All the cultures from A show a growth, while those from B are sterile.

EXPERIMENT IV.—A mixture was made of cream 50 c. cm., milk 25 c. cm., lime water 50 c. cm., and milk sugar solution[1] 75 c. cm; this was divided into two lots, thirteen test-tubes in all, and treated as in III.

[1] Milk sugar ℥17¾, water Oj.

10th day. Mixture turned brown at once upon steaming, and so remained; no other visible change. Agar-agar cultures, with one loop made from each.

20th. No change in test-tubes; cultures both A and B sterile.

As Dr. T. M. Rotch[1] reports no changes in the cream mixture after steaming, I made the following experiments to discover the source of the brown color in IV.

Experiment V.—A mixture of one part milk, two cream, and three milk sugar solution was placed in seven test-tubes and steamed for fifteen minutes.

2d day. No brown color, cream very thick.

8th. No visible change; reaction to litmus paper neutral. Agar-agar culture made from each tube.

23d. No changes in the tubes; cultures all sterile.

Experiment VI.—A mixture of equal parts of cream, milk, and lime water, placed in six test-tubes, and steamed for fifteen minutes.

2d day. No brown color; cream on top.

8th. Agar-agar cultures made.

23d. Tubes all fresh; cultures all sterile.

Experiment VII.—First boiled lime water and milk sugar solution separately, over a Bunsen burner, and found no change except that lime was left on the side of the test-tube. Next made a mixture of the two, in the proportion of two to three, and divided it up among ten test-tubes. Reaction strongly alkaline. The ten tubes were then put in the steamer. One was taken out in five minutes, another in ten, a third in fifteen, and the rest at the end of a half hour; all were found to be brown, or rather a yellow-brown, the first slightly less so than the rest. They were then tested with litmus paper, and found to show a marked diminution in alkalinity.

No further experiments were made, as the above seemed to show clearly that the brown color was due to the action of the lime on the milk sugar, like that produced by potassic hydrate, in which case brown products are formed. However this may be, the reaction always took place in my experiments, provided the milk sugar had not become decomposed, as it shortly does if fungi get into the solution.

Why my results should thus vary from Dr. Rotch's is not clear, unless it be that his mixtures had been mixed before he saw them. If examined before disturbing, the cream above is quite light, and the thin fluid below distinctly brown, after mixing the cream makes the whole quite opaque, and the color is almost concealed. It will be remembered Dr. Rotch did not report a test of the reaction after steaming.

In the following experiments the flasks were put in the hot steamer, instead of into the cold steamer and heated up. This was done in order to adopt as much as possible the principle of Pasteurisation; a rapid being more fatal than a gradual change of temperature of the same extent.

Experiment VIII.—Cream mixture of the same proportions as in IV. was put in eighteen test-tubes, and steamed for fifteen minutes. On the following day nine of them (B) were steamed again for fifteen minutes. All of the tubes, both A and B, became brown during the steaming, and, as was later found, lost much of their alkalinity.

5th day. Agar-agar cultures made from three of A and three of B; milk tested with litmus paper; B found neutral, A faintly alkaline.

9th. One of the agar-agar cultures from A shows a growth.

[1] The Archives of Pediatrics, 1887, vol. iv. p. 462.

15th. Another from A shows growth, all B are sterile.

EXPERIMENT IX.—A mixture of Anglo-Swiss condensed milk one part, hot water nine parts, was put in thirteen test-tubes and placed in the hot steamer for fifteen minutes; on the next day seven of these (B) were again steamed for fifteen minutes.

4th day. Cream on top, milky fluid below and slight sediment at the bottom, but no visible signs of decomposition. Three agar-agar cultures made from each A and B.

20th. All the agar-agar tubes sterile.

EXPERIMENT X.—To a mixture of Anglo-Swiss condensed milk like the last was added an old culture of a bacillus producing putrefaction. All twenty tubes were then placed in the hot steamer. Four were removed in five minutes (A), four in ten (B), four in fifteen (C), and the remaining eight in twenty (D).

3d day. Agar-agar tubes inoculated from two tubes of each lot of milk, making eight in all.

8th. One of A is negative, the other shows a good growth; B, C, and D, sterile.

16th. The other A and one of the C tubes have taken, rest sterile. Ultimately all the milk tubes but one steamed for twenty minutes decomposed.

EXPERIMENT XI.—Fifteen test-tubes were filled with milk and put in the hot steamer, they were removed in lots of five at the end of five, ten, and fifteen minutes. At the same time three gelatine Esmarch tube cultures were made, each containing two drops of the fresh milk.

2d day. Agar-agar cultures made with one loop from each of the tubes of milk.

3d. The Esmarchs contain about 250 colonies each.

14th. All the agar-agar tubes are sterile, but the milk has coagulated in all but three tubes.

EXPERIMENT XII.—This experiment was the same as the last, except that the agar-agar tubes were inoculated from the milk four days after steaming instead of one day.

8th day. All five of those steamed for five minutes show growth, also one of those steamed for ten.

12th. Three more of those steamed for ten minutes show growth. All the milk tubes steamed for five or ten minutes have coagulated, also one of those steamed for fifteen minutes. No further changes occurred.

EXPERIMENT XIII.—Fifteen test-tubes were steamed as in the last two experiments and stood aside to be tested for their bacterial contents at the end of fourteen days. At the end of this time all of the tubes but one of those steamed for fifteen minutes had clotted, so no cultures were made.

EXPERIMENT XIV.—Three flasks each containing 100 c.cm. of milk were put in the hot steamer for fifteen minutes.

2d day. Agar-agar cultures were made from the flasks.

32d. Agar-agar tubes still sterile.

EXPERIMENT XV.—This experiment was the exact counterpart of Experiment XIV. and gave the same results.

EXPERIMENT XVI.—One flask containing 200 c. cm. of milk was put in the hot steamer for fifteen minutes.

2d day. Agar-agar tubes inoculated with one loop from the flask.

30th. Agar-agar tubes sterile.

EXPERIMENT XVII.—Four tubes each containing 100 c. cm. of milk were steamed for fifteen minutes.

7th day. Milk acid in reaction, but shows no visible change. Agar-agar tube inoculated from each flask by a loop.

12th. One of the agar-agar tubes shows a good growth.

20th. Another of the agar-agar tubes has taken.

30th. The other two agar-agar tubes are still sterile.

EXPERIMENT XVIII.—Three flasks each with 100 c. cm. of milk were steamed for fifteen minutes.

2d day. Three agar-agar tubes inoculated, with a loop, from the three flasks; milk also tested with litmus paper and found to be faintly acid.

15th. The three agar-agar tubes are sterile.

Experiment XIX.—A test-tube of the lot of milk used in the last was set aside for a day, and then three agar-agar tubes inoculated, each with one loop.

4th day. The agar-agar tubes are simply swarming with bacteria.

Experiment XX.—Five agar-agar tubes were inoculated with one loop each from the lot of fresh milk used in Experiment XVIII.

5th day. All show good growth with isolated colonies on the edge.

If we stop to review the results of the above experiments it is at once seen that milk cannot often be sterilized by one steaming. Of the one hundred and twenty odd lots of milk steamed but once, all but four or five showed distinct signs of change within a month. Two which appear sterile are, however, still in my possession after twice that lapse of time. How it happened that these few are sterile will be explained later on. On the other hand, the majority of those steamed twice did not change at all; those which did change, it may be added, coagulated at about the same time as those steamed but once.

But if we look over the data of the agar-agar tubes inoculated by the loop, we see that such may be sterile even if made from milk which has stood for a long period. This is shown by Experiments I., II., III., and V., where the milk had stood from seven to ten days after being steamed before the culture was made. That the loop was sufficiently large to carry enough milk to contaminate the agar-agar is shown by XX. No more such experiments are reported, though many were made in the course of other work, as all showed the loop to contain quite a number of bacteria.

Turning to the twenty agar-agar tubes inoculated from milk which had stood one day, we find that all failed to show any growth. It was evident, therefore, that steaming for fifteen minutes offered good prospects of success.

Before passing to my final experiments to elaborate this point, it must be noted that on several occasions the growth of bacteria on the agar-agar had not become distinctly visible until the tenth day.

The following experiments go in threes: a gelatine Esmarch culture was made with one drop of milk or mixture used, then the flasks, each containing 100 c.cm. of the substance, were steamed for fifteen minutes; and lastly, a part was set aside stoppered in the room. As soon as the flasks were steamed they were put beside the one not steamed. At the end of twenty-four hours Esmarchs were made from all, one drop being taken by a sterile pipette. By this means it was possible to count the number of bacteria in a drop of the substance, a large quantity, before steaming and a day after it had been steamed, and to gather an idea of what the number would have been if it had not been steamed.

EXPERIMENT XXI.—Four flasks were filled, each with 100 c. cm. of milk, and then steamed at once for fifteen minutes, after which they were stood away for a day. At the end of this time four Esmarchs were made from the four flasks, with one drop each.

3d day. No signs of growth in the Esmarchs.

11th. The Esmarchs show 0, 1, 3, and 10 colonies respectively, all of one kind.

EXPERIMENT XXII.—Two Esmarchs were made with one drop each from the fresh lot of milk used in the last.

7th day. Calculations made in the usual way by counting the colonies in a given area, and multiplying by the total area give 1644 and 1391 colonies.

EXPERIMENT XXIII.—Steamed three flasks, each with 100 c.cm. of milk for fifteen minutes.

2d day. Made Esmarchs with one drop from each of the three flasks.

9th. All sterile.

29th. All sterile.

EXPERIMENT XXIV.—A test-tube filled from the lot of milk used in the last was set aside till the following day, when two Esmarchs, with a drop each, were made at the same time as those of XXIII.

4th day. Esmarchs entirely dissolved by the minute colonies, thus showing the presence of innumerable bacteria in the milk.

EXPERIMENT XXV.—One gelatine test-tube was inoculated with one drop of the fresh milk, and should have been rolled out into an Esmarch but was forgotten.

4th day. Full of colonies, but it is impossible to count them in the mass of gelatine.

9th. Gelatine all dissolved for some time.

EXPERIMENT XXVI.—Mixture made of 7 grm. Mellin's food and 150 c. cm. of hot water, after the food was dissolved, 150 c. cm of milk were added, and the whole steamed. Two flasks were then filled with 100 c. cm each, and steamed for fifteen minutes.

2d day. Esmarchs, one drop made from each.

5th. Each Esmarch contains one colony.

19th. No change.

EXPERIMENT XXVII.—Two Esmarchs made, with one drop each, from the fresh lot of Mellin's food used in the last experiment.

3d day. One Esmarch shows 18, the other 20 colonies.

8th. They now show 30 and 34 colonies.

15th. No change in the numbers.

EXPERIMENT XXVIII.—Sample from the mixture used in Exp. XXVI. set aside until the next day, when an Esmarch with one drop was made.

3d day. Esmarchs contain so many minute colonies it is impossible to count them; the whole shortly dissolved.

EXPERIMENT XXIX.—A mixture was made of one part Anglo-Swiss condensed milk and ten parts hot water. From this two flasks, 100 c. cm. each, were filled and steamed for fifteen minutes.

2d day. Esmarch, with one drop, made from each.

5th. No colonies visible.

12th. One Esmarch is sterile, the other has one colony.

16th. No new colonies.

EXPERIMENT XXX.—Two Esmarchs were made from the fresh mixture used in the last experiment, each with one drop.

5th day. The Esmarchs show 646 and 612 colonies, according to calculation; these shortly dissolved the gelatine.

EXPERIMENT XXXI.—A test-tube full of the condensed milk solution used in Experiment XXIX. was set aside for a day, when an Esmarch with one drop from it was made.

5th day. The fourth day of the Esmarch, calculation derived from counting the colonies in a small square with a strong lens gave 9750.

EXPERIMENT XXXII.—A mixture of seven grms. Mellin's food, hot

water, and milk, each 150 c. cm., was carefully made and steamed. Two flasks were then filled with 100 c. cm. each and steamed for fifteen minutes.

2d day. Esmarchs, one drop, made from each.

4th. One Esmarch shows one colony, other none.

15th. No new colonies.

Experiment XXXIII.—Esmarch made with one drop of the fresh mixture used in the last experiment.

4th day. Thirty colonies.

10th. Forty colonies. No new colonies occurred later.

Experiment XXXIV.—Esmarch with one drop of the mixture used in the last two experiments, after standing for a day. By this time the top was brown and dirty looking.

2d day. The whole is dissolved.

Experiment XXXV.—One flask was filled with 100 c. cm. of milk and steamed for five minutes.

2d day. Esmarch with one drop made.

13th. Two colonies.

Experiment XXXVI.—An Esmarch was made with one drop of the fresh milk used in the last experiment.

4th day. Sixty colonies by count.

Shortly dissolved in places, so no later count could be made.

Experiment XXXVII.—A sample of the milk used in the last two experiments was set aside for a day; then an Esmarch made with one drop.

3d day. Esmarch all dissolved.

Experiment XXXVIII.—Two flasks of milk, each 100 c. cm., were steamed for ten minutes.

2d day. Made an Esmarch with one drop from each.

4th. No colonies in the Esmarchs.

18th. No colonies.

Experiment XXXIX.—Esmarch with one drop of the fresh milk used in the last experiment.

4th day. Careful count gave 84 colonies. All dissolved in a few days.

Experiment XL.—A sample of the milk used in the last two experiments was set aside for a day, when an Esmarch was made with one drop from it.

4th day. Esmarch dissolved.

Experiment XLI.—A mixture of cream 70 c. cm., milk 35 c. cm., lime water 70 c. cm., sugar and lime water 105 c. cm. made. From this, two flasks were filled with 100 c. cm. each, and steamed for fifteen minutes.

2d day. Esmarchs made with one drop from each.

9th. No colonies.

17th. Esmarchs sterile.

Experiment XLII.—Esmarch made with one drop of the fresh mixture used in the last experiment

3d day. About 150 colonies.

7th day. Gelatine dissolved.

Experiment XLIII.—A sample of cream mixture set aside.

2d day. Esmarch with one drop made.

3d. Looks to be crowded with colonies.

7th. All dissolved.

Experiment XLIV.—From a mixture of one part Anglo-Swiss condensed milk and ten parts water, two flasks were filled with 100 c. cm. each and steamed for fifteen minutes.

2d day. Esmarch with one drop made from each.

17th. Both Esmarchs are still sterile.

Experiment XLV.—Esmarch with one drop made from the fresh mixture used in the last experiment.

3d day. Nothing visible yet.

13th. About 200 small and 2 large colonies.

Experiment XLVI.—A sample of the mixture, used in the last two, set aside for a day, when the customary Esmarch was made.

9th day. Two-thirds dissolved by about forty large colonies, solid parts stippled with pin-point colonies.

EXPERIMENT XLVII.—A cream mixture was made in the same proportions as before, and two flasks, 100 c. cm. each, filled from it; the flasks were then steamed for fifteen minutes.

2d day. Esmarch made as usual.

8th. No colonies to be seen.

16th. Both Esmarchs sterile.

EXPERIMENT XLVIII.—An Esmarch with one drop of the fresh cream mixture was made.

2d day. Several minute colonies.

8th. Esmarch dissolved.

EXPERIMENT XLIX.—A sample of the cream mixture used in the last two was stood aside for a day, when an Esmarch with one drop was made.

8th day. Dissolved.

I have put the results of the later experiments in a table, to help comparison. The *a* sign indicates that a large number of colonies developed and dissolved the gelatine before they had grown large enough to count.

Experiment, and substance used.	Quantity.	Time steamed.	Time stood.	No. of bacteria.	Experiment, and substance used.	Quantity.	Time steamed.	Time stood.	No. of bacteria.
	c. m.	seconds.				c. m.	seconds.		
XXI. Milk.	100	15	1 day.	0	XXXIII. Mellin's.	......	......	......	40
Milk.	100	15	1 "	1	XXXIV. Mellin's.	......	......	1 day.	*a*
Milk.	100	15	1 "	3	XXXV. Milk.	100	5	1 "	2
Milk.	100	15	1 "	10	XXXVI. Milk.	......	......	......	60
XXII. Milk.	......	......	......	1644	XXXVII. Milk.	.....	......	1 day.	*a*
Milk.	......	......	......	1391	XXXVIII. Milk.	100	10	1 "	0
XXIII. Milk.	100	15	1 day.	0	Milk.	100	10	1 "	0
Milk.	100	15	1 "	0	XXXIX. Milk.	......	......	......	84
Milk	100	15	1 "	0	XL. Milk.	......	......	1 day.	*a*
XXIV. Milk.	......	......	1 "	*a*	XLI. Cream milk	100	15	1 "	0
Milk.	......	......	1 "	*a*	Cream milk.	100	15	1 "	0
XXVI. Mellin's.	100	15	1 "	1	XLII. Cream milk.	......	......	......	150
Mellin's.	100	15	1 "	1	XLIII. Cream milk.	......	......	1 day.	*a*
XXVII. Mellin's.	......	......	......	30	XLIV. Cond. milk.	100	15	1 "	0
Mellin's.	......	......	......	34	Cond. milk.	100	15	1 "	0
XXVIII Mellin's.	......	......	1 day.	*a*	XLV. Cond. milk.	......	......	......	202
XXIX. Cond. milk	100	15	1 "	1	XLVI. Cond. milk.	......	......	1 day.	*a*
Cond. milk.	100	15	1 "	0	XLVII. Cream milk.	100	15	1 "	0
XXX. Cond. milk.	......	......	......	646	Cream milk.	100	15	1 "	0
Cond. milk.	......	......	......	612	XLVIII. Cream milk.	......	......	......	*a*
XXXI. Cond milk.	......	......	1 day.	9750	XLIX. Cream milk.	......	......	1 day.	*a*
XXXII. Mellin's.	100	15	1 "	1					
Mellin's.	100	15	1 "	0					

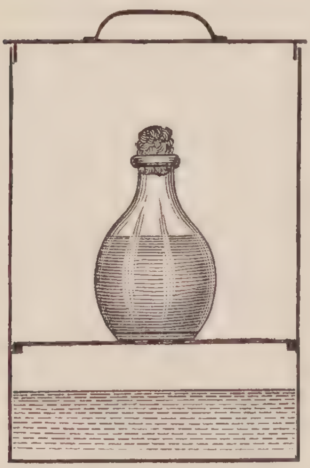
In looking over the table it is at once seen that very few or no colonies developed from the material steamed. Out of twenty-two trials, in eight bacteria were found, in fourteen none was found. Of the eight cases, only one colony each was found in five.

The calculations from the fresh mixture before steaming show from 30, in a mixture of Mellin's food, to 1644, in a sample of milk; the average for the milk being about 75. In the one case where a count was secured from the mixture after standing a day we found 9750. Very likely the others contained more.

It is, therefore, clear that the method followed has been very successful in killing bacteria, and keeping the milk for a long period. We may thus lay down the following rules in answer to our problem: Stopper the flasks with cotton-wool and heat them in the oven for thirty minutes, at a mild baking heat, or until the wool becomes brown. Pour the requisite quantity of food into the flask and then place in the heated steamer for fifteen minutes.

The first rule is an advantage, and easily done, but not of great importance. The second is both easily done and goes to the root of the subject. Any cooking steamer with a perforated false bottom and a snug cover will do; or the lower part of a Chamberlin's steamer; the heat must be sufficient to keep the water in active ebullition.

For the benefit of those who are not familiar with the technique of bacteriology, or familiar with steamers, a diagram of a section is given, showing the principles and essential elements of construction. The vessel should be of good size, at least eight inches across the bottom —better a foot, and sixteen inches high. Inside, four inches from the bottom, there should be a projecting rim, on which should rest a metal plate perforated with numerous holes a half inch in diameter. The cover should be tight so as to hold in the steam and prevent the ingress of air. For use, two or three inches of water should be placed in the bottom and brought to a fast boil, when the flasks should be set as near the centre of the diaphragm as possible, the cover replaced, and the whole allowed to steam for fifteen minutes. The flasks should then be taken out and stood in the cold; of course, to be brought up to the body temperature before feeding.



The chief source of failure lies in an insufficient supply of steam to keep the upper chamber full; the heat must be ample, that of a range or Bunsen burner. Where the additional expense can be borne, it is better to cover the outside of the steamer with a thick jacket of felt, extending to within two inches of the bottom.

The milk should be steamed when *first* received, preferably in the flasks from which it is fed to the infants. This requires a few more bottles, as many as the infant is fed times during the day, but will well repay for the trouble. If the milk is allowed to stand before steaming, the advantages of the method are done away with in great part. The milk may be sweet, but has already been acted upon by bacteria, and is certainly unhealthy. In case a sufficient number of flasks cannot be afforded, the milk should be steamed in a few larger ones, kept stoppered with cotton-wool, and drawn from as needed. This is the best method to employ in hospitals, where the contents of a large flask will be used up in a short time.

The secret of the success of this method lies in the well-known fact that the vegetative forms of bacteria succumb to a moist temperature of 100° C. (212° F.); that spores develop slowly; and lastly, but not least, that in milk, being an excellent medium for growth, spores rarely form, spore-formation among bacteria, like seeding among higher plants, being a phenomenon of impaired growth. The dearth of spores in ordinary milk can be demonstrated by the use of the microscope and patience.

Fifteen minutes' steaming is advised rather than five or ten, as some of the earlier experiments reported show the longer period to be more effective. The entire mass of fluid used must be heated up to the boiling-point; for this time is requisite; it is not without significance that the fifteen minutes' steaming is that employed by bacteriologists to sterilize their media.

The preservation of some of the milk steamed but once is explained by the absence of any enduring spores from the start.

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THE REINFORCEMENT AND INHIBITION OF THE KNEE-JERK.[1]

BY H. P. BOWDITCH, M.D.,
Professor of Physiology, Harvard Medical School.

THE familiar fact that a blow upon the ligamentum patellæ causes a sudden contraction of the extensor muscles of the thigh became an object of careful physiological study when its diagnostic importance was pointed out by Erb and Westphal.

One of the most important additions to our knowledge of the phenomenon was made about two years ago by Drs. S. Weir Mitchell and Morris J. Lewis,[2] who, directing their attention to a phenomenon first pointed out by Jendrássik,[3] showed, in a series of carefully conducted experiments: (1) That the so-called knee-jerk can be increased by volitional acts directed to other parts of the body; (2) that volitional reinforcement lasts for an appreciable time after volition ceases; and (3) that continued violent muscular acts at last enfeeble the knee-jerk, and this enfeeblement lasts for an appreciable time.

These conclusions suggested the importance of studying the exact relations in time between the knee-jerk and the reinforcing act since we might reasonably hope to obtain in this way some insight into the nature of the mysterious processes having their seat in the central nervous system; and the object of this communication is to present to the Academy a preliminary report of the results of some experiments recently made in the physiological laboratory of the Harvard Medical School by Dr. J. W. Warren and myself with the hope of throwing light upon this subject.

[1] Read at a meeting of the National Academy of Sciences at Washington, April 17, 1888.
[2] Physiological Studies of the Knee-Jerk, etc. The Medical News, Feb. 13 and 20, 1886.
[3] Deutsches Archiv für klinische Medicin, Vol. XXXIII, p. 175.

A detailed description of the apparatus used need not be given in this connection. Suffice it to say that the blow upon the ligamentum patellæ was delivered by a light hammer, (Fig. 1, H) fixed upon a splint-like covering enclosing the lower leg. By this arrangement a certainty that the hammer always struck the same point during each experiment was secured. The force of the blow was regulated by the tension of a spinal-spring S, which drew the hammer toward the knee when it was released by the breaking of an electric current controlling a magnet, M, also attached to the covering of the leg. The straps B B B served to secure the apparatus to the leg, and the rings L L to suspend the leg from the ceiling as shown in Fig. 2.

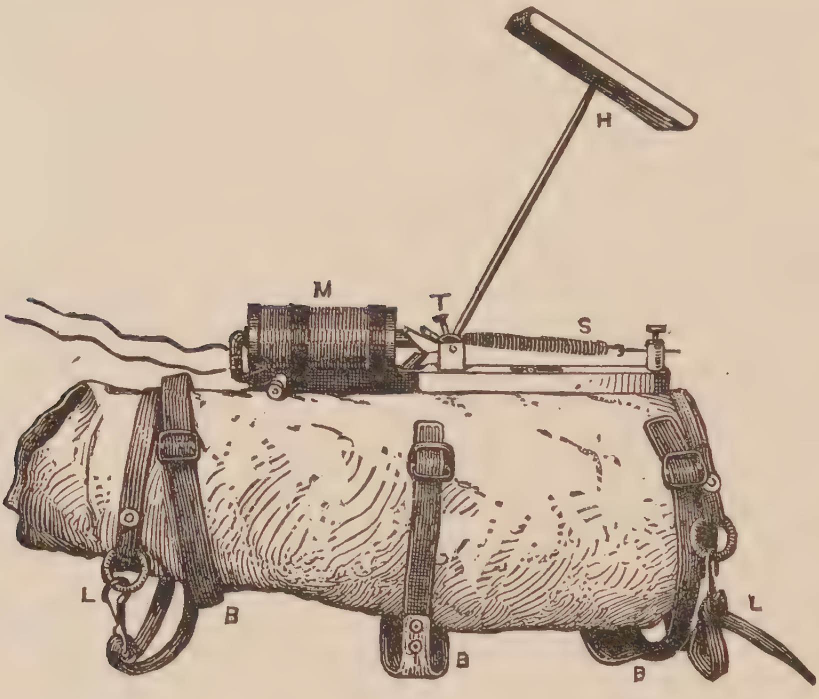

Figure 1.

The individual to be experimented upon lay upon his right side, with the left knee slightly bent (Fig. 2), and the internal condyle of the left femur resting upon a fixed support, the position being essentially the same as that adopted by Lombard.[4] The weight of the lower leg was borne by a cord hanging from the ceiling and fastened to the splint-like apparatus on the leg. The lower leg was thus free to swing in a horizontal plane round the knee-joint as a pivot, and its movement, reduced by a system of levers to one-sixth of its extent, was recorded by a pen (P) upon

[4] Is the "knee-kick," a reflex act? Am. Journ. of Med. Sciences, January, 1887.
The Variations of the Normal Knee-Jerk. The Am. Journal of Psychology, Vol. I, p. 1.

the smoked surface of a cylinder revolving once an hour. A pendulum myograph (D) served to break two electric circuits as it swung by striking against two keys adjustable at various points in its course. One of these circuits controlled the magnet holding up the hammer at the knee, and the other an electric bell (B), a stroke of which, on the breaking of the circuit, was the signal for the reinforcing act. Thus, by varying the position of the keys, it was possible to secure any desired interval from 0.0″ to 0.5″ between the reinforcing act and the knee-jerk. Where longer intervals were required, a pendulum vibrating more slowly than that shown in the figure was employed.

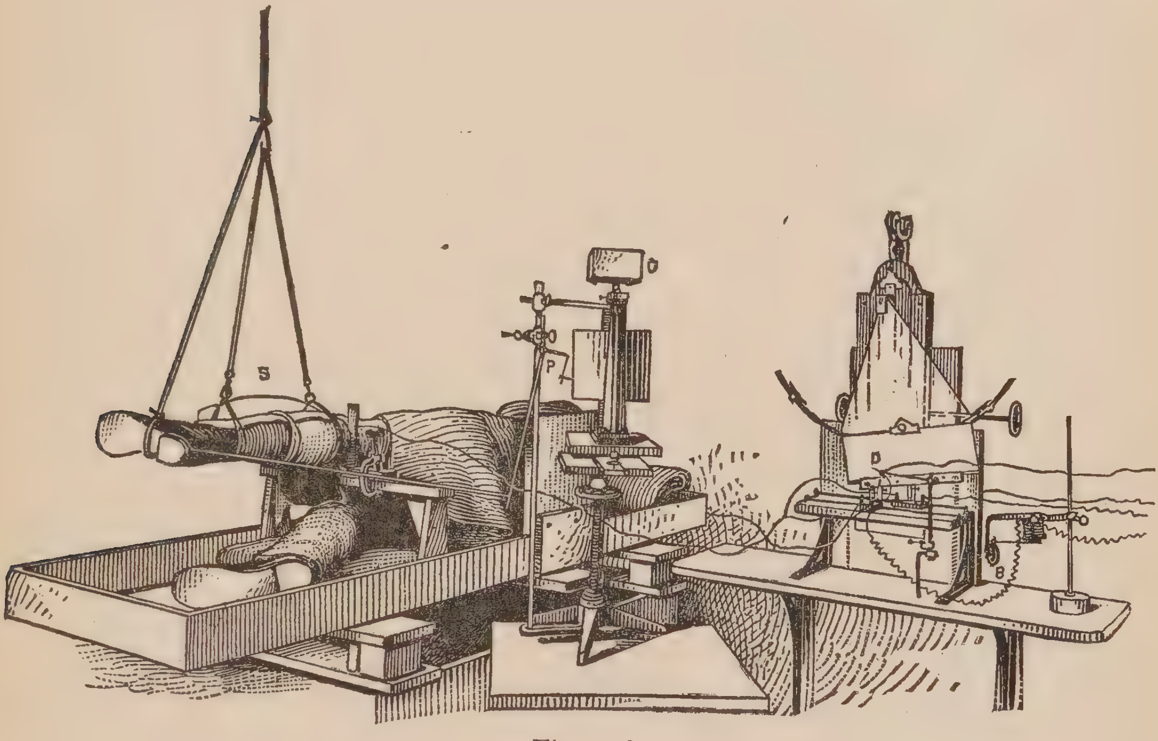

Figure 2.

The reinforcing act was a short, vigorous, clinching of the right hand upon a piece of wood, shaped somewhat like a tuning-fork, and furnished with metallic tips so adjusted that a slight pressure would bring them into contact, and thus close the same electric circuit that was opened when the bell was struck. As this circuit also included a Deprèz signal-magnet writing upon the smoked surface of the pendulum myograph, a record was obtained with each experiment of the reaction-time of the individual to an auditory impression.

This reaction-time, of course, includes a centripetal, a central, and a centrifugal portion, and as the total

duration was liable to considerable variation, and as, moreover, it was impossible to say in advance which portion of the reinforcing act would have the greatest influence upon the knee-jerk, it was decided to regard the stroke of the bell as the zero point of the reinforcing act, and when this point coincided in time with the blow of the hammer upon the ligamentum patellæ, the interval between the reinforcement and the knee-jerk was called zero. The object of the investigation was, therefore, to ascertain how the extent of the knee-jerk would be affected by varying the interval of time at which the blow upon the ligamentum patellæ followed the signal for reinforcement. No experiments were made in which the blow preceded the signal, since it would be difficult, after receiving the blow, to wait for the auditory signal before giving the reinforcing act.

Each experiment lasted, as a rule, about one hour. During this time several series of observations were made, each with a different interval between the bell-signal and the blow. Each series was divided into two portions, in the first of which there was no reinforcing act, and the knee-jerk was regarded as "normal," while in the second the individual responded to the bell-signal in the above-mentioned manner. The difference between the average extent of the knee-jerk in the first and second portion of each series was called the *special* reinforcement for the interval corresponding to that series, and the difference between the extent of the knee-jerk in the second portion of each series and in the first portion of *all* the series in the same experiment was called the *general* reinforcement for the same interval.

Experiments were made upon four different individuals with the same general results in each case. The nature of this result may be best understood by an examination of the curve shown in Figure 3, constructed from the record of 551 normal, and 624 reinforced knee-jerks in the same individuals. In this curve the abscissas represent the intervals between the bell signal and the blow, and the ordinates the difference between the "normal" and the re-inforced knee-jerks. Positive ordinates indicate an increase, negative a diminution of the knee-jerk. The figures at the left of the curve show in millimetres the absolute amount of increase or diminution. The dotted curve is the curve of *special* reinforcement; that is, it represents the average difference between the reinforced knee-jerk and the *special* normal of that series, while the full curve is the curve of *general* reinforcement represent-

ing the average difference between the reinforced knee-jerk and the general normal of the experiment. Both curves follow, it will be observed, the same general course, and show clearly that if the blow follows the signal at an interval not greater than 0.4″ the reinforcing act increases the extent of the knee-jerk. If the interval exceeds this amount a diminution

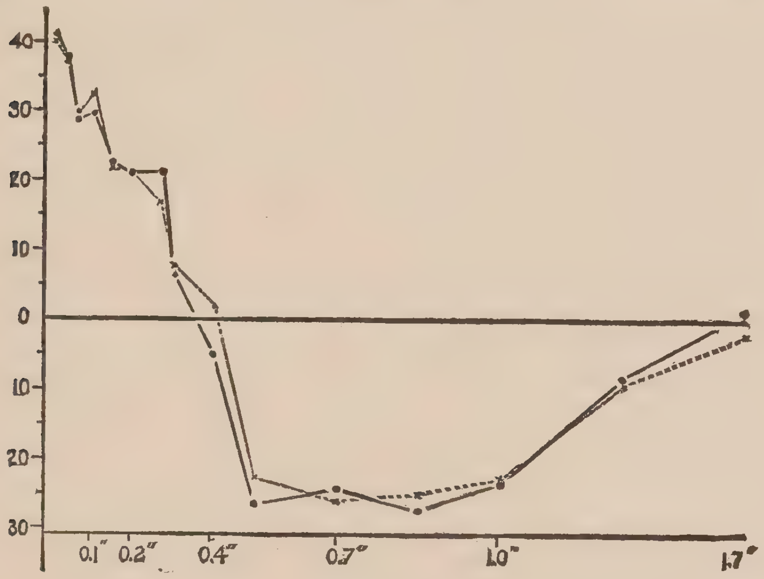

Figure 3.

of the knee-jerk results. If, however, the interval is prolonged to 1.7″ the reinforcing act is without effect upon the knee-jerk. To express the same conclusion in other words, we may say that, when by a brief act of volition the muscles of the forearm are innervated, the spinal cord is thrown into such a condition, that that portion of it which is concerned in the production of the knee-jerk is for a short time in a state of exalted activity which is succeeded by a period of depression and then by a slow return to the normal state.

We thus find the activity of one set of nerve centres serving first to exalt and then to depress the activity of a neighboring set. Without speculating upon the nature of the processes thus reacting upon each other it may be well to point out that we have in this alternating action a phenomenon which cannot fail to throw light upon the nature of "inhibition" and destined perhaps, when fully understood, to establish the interference theory on a firm basis.[5]

In this connection also, should be noted the observations of Meltzer on the inhibition of the peristaltic movements of the œsophagus by a rapid succession of movements of deglutition.

[5] Cf. Mitchell, op. cit., Reprint, p. 34.

It is evident that a wide field of research is open and that the various modes of activity of the central nervous system should be studied with reference to their effect upon the knee-jerk, and it is not too much to hope that careful experimental work directed on these lines will dispel a portion of the mystery which now surrounds the function of the nerve-cells.

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VOLUNTARY CONTROL OF THE HEART.[1]

BY EDWARD ALLEN PEASE, A.B. (HARVARD).

ALTHOUGH the heart has always been considered one of the most involuntary organs connected with the human body we have now evidence that there are gifted individuals who have a certain amount of direct control over it. The wonderful connection between the heart's action and a person's feelings is well known. Any feeling of an agreeable or joyful character will tend to increase the rate of the heart-beat, whereas those feelings of a disagreeable or sad nature will tend to decrease it. Moreover, a very strong or sudden emotion of any sort may cause a momentary stopping of the heart-beat, as most people know by experience. Physiologists have long been aware of the close relationship between the heart's action and that of the brain; yet, for lack of sufficient evidence, have not granted that any direct control over the heart could be induced by a simple effort of the will.

In treating the subject of voluntary control of the heart I shall divide it, to avoid confusion, into two parts: 1. Indirect voluntary control, due to the effort of the will directed upon some object or objects which, in their turn, produce an effect upon the heart's action. 2. Direct voluntary control, due simply and purely to an effort of the will directed upon the heart itself.

1. Indirect voluntary control of heart appears in several forms, some of which I will mention with examples, in order that we may have a clear idea of this sort of control before we enter upon the question of direct voluntary control, which forms the special subject of this paper. (*a*) Voluntarily induced sad emotions have been known in several instances to bring about a change in the heart's rhythm. Professor Botkin[2] states he had once a patient under treatment for atrophy of her muscles who could diminish the beat of her heart at pleasure

[1] Read at a physiological conference in the Harvard Medical School, February 4, 1889.

[2] Klinische Wochenschrift. Herausgegeben von Botkin, 1881, No. 10.

and for as long a time as she wished, and, moreover, could destroy its regular rhythm. In order to do this she had only to look at the bed in which she was formerly ill and thus bring into her mind the unpleasant associations surrounding it. When in good spirits she had no control over her heart. (*b*) Pressure on the vagus (a large nerve running alongside the carotid artery in the neck, and, on its way to the stomach, sending inhibitory branches to the heart), has been brought into service to cause a decrease in number of heart-beats. Professor Czermak,[3] of Prague, could diminish the rate of his heart-beat more than one-half by simply pressing on this nerve. As he had an enlarged indurated lymph gland lying just beneath the vagus nerve in the upper portion of his neck he found no difficulty in bringing about this pressure. (*c*) Checking the respiratory rhythm by a strong contraction of the thoracic muscles while the glottis is closed in inspiratory position has been sufficient to stop the heart-beat. Weber[4] has performed this experiment successfully on himself, and explains this decrease in the rate and stoppage of the heart's beat, partly by a lessening of the pressure within the thorax and partly by a storing up of carbonic dioxide gas by means of which the inhibitory heart-centres going to the medulla oblongata become irritated. But Wertheimer and Meyer[5] explain a similar case by saying the diminution in the number of beats was due to irritation of the threads of the vagi which run to the lungs. These threads become pressed together by the full alveoli and so are stimulated. (*d*) A marked change made in the respiratory rhythm will cause a change in the rapidity of the heart's beat; but an increased number of heart-beats from this cause would really come under the head of increased heart-action due to increased muscular action of any sort, — a phenomenon with which we are all familiar.

2. Direct voluntary control of heart. Carpenter[6] says that the influence of the state of expectant attention is strangely manifested in the heart; the action of which, Sir H. Holland has remarked, "is often quickened or otherwise disturbed by a mere centering of consciousness upon it without any

[3] Jenaische Zeitschrift f. Med. und Naturwiss., 1865, p. 384.
[4] Weber. Ueber ein Verfahren, den Kreislauf des Blutes und die Function des Herzens willkürlich zu unterbrechen. Arch. f. Anat. und Physiol. und wissenschaftl. Medicin, von T. Müller, 1851, p. 88.
[5] Archives de Physiologie, Paris, 1889, p. 49.
[6] Human Physiology, Carpenter, 1869, p. 808.

emotion or anxiety; and where there is a liability to irregular pulsation such action is seemingly brought on, or increased, by the effort of the attention, even though no obvious emotion be present." We know that the vagus nerve has inhibitory and the sympathetic nerve accelerator action upon the heart. A voluntary control apparently from the action of one or the other of these nerves can thus be effected by certain persons who possess the requisite amount of will-power.

Joseph Frank[7] made his heart's beat intermittent and irregular by simply concentrating his attention upon it. A Bologna professor[8] was completely cured of intermittent pulse by following the advice of his physician, Morgagni, not to count his pulse, *i.e.*, not to pay any more attention to it. There was a Fellow of the Royal Society in London[9] who could increase his heart's beat ten to twenty times a minute by a simple effort of the will.

The best known and most historic case of voluntary control of heart is that of Lieutenant Townsend.[10] He possessed the power to stop at will his heart-beat and breathing, and at the same time to go into a deathlike sleep. His body would begin to cool and stiffen, his eyes become immovable, and finally his consciousness almost leave him. In a few hours, however, he would return to full consciousness. One evening, after he had made an experiment before a large audience to show his power of voluntary control of heart, this man died. From the fact that Lieutenant Townsend died soon after the only experiment of which we have an accurate description it might be thought that he was already in a moribund condition before trying this experiment; yet the post-mortem examination revealed a diseased condition in no part of the body except one kidney. In all the cases I have mentioned so far there is no conclusive evidence that the change in the heart's rhythm was due simply to direct voluntary control. To prove this all forms of indirect voluntary control must be excluded.

It was by the aid of the sphygmograph, pneumograph, plethysmograph, and other instruments that Professor Tarchanoff,[11] a Russian physiologist, was

[7] Joseph Frank. Praxæ medicæ universæ præcepta. Lipsiæ. Th. ii. Bd. ii. Abth. ii. p. 373.

[8] Wagner's Handwörterbuch d. Physiol., 1844, Bd. ii. p. 82.

[9] D. H. Tuke. Illustrations of the Influence of the Mind upon the Body in Health and Disease, London, 1872.

[10] Carpenter's Human Physiology, 1853, p. 1103.

[11] Ueber die Willkürliche Acceleration der Herzschläge beim Menschen. Archiv für die Gesammte Physiologie, Bd. 35, p. 109.

able to prove scientifically that in certain cases the heart is under the direct control of the will.[12] He found among his students a young man named Salomé, who claimed to have voluntary control over his heart. The young man was of a rather nervous temperament, and, when ten or fifteen years of age, had suffered from palpitation of the heart without any apparent cause. In course of time, however, he had rid himself of the disease and would have forgotten all about it, were it not for the fact that he noticed accidentally he was able to change the rhythm of his pulse by a comparatively trifling outside cause. Experimenting further he found he could accelerate his pulse by simply concentrating his attention upon it and exerting a sufficient amount of will-power. It was at this time that Salomé came under the attention of Professor Tarchanoff.

At the first meeting Salomé gave a striking proof of his ability by accelerating the beat of his heart from 70 to 105, *i.e.*, 35 beats a minute. The experiment was repeated several times with about the same result, although the amount of the acceleration fell off plainly with each repetition of the act. In later experiments Salomé increased the beat somewhat more. He found that arsenic would assist him in accelerating the heart's beat, and that nitrous oxide (laughing gas), although increasing the heart-beat 6 or 8 a minute by its own properties, would at the same time take away the power of the subject to control his heart voluntarily. When he recovered from the effects of the nitrous oxide he was again able to accelerate the beat 25 a minute. Salomé said he called up no extraneous ideas to increase his heart's beat, but used a special effort of the will. Throughout the experiments Tarchanoff found that the respiration during acceleration was not quite normal, as may be seen by inspection of the pneumographic tracing marked 3 in Fig. I., a reproduction of one of Tarchanoff's figures. Tracing 2 shows the period of acceleration by a heavy white line made by the subject pressing gently an electric button during the time he was trying to increase his heart-beat. Tracing 1 is made by a galvanic chronograph marking seconds. Tracing 4 was obtained by placing the foot in a plethysmograph, a rubber cap arrangement fitting tightly over the leg and having the air space between the

[12] I am indebted to Professor Tarchanoff's article for several of my references.

foot and the cap connected with a Marey drum. It shows the increased heart-beat during the acceleratory effort and the diminution in the size of the limb as appears from the general falling of the line. This diminution is associated with an increased blood-pressure, which lasts after the effort at acceleration has ceased, thus showing that a vaso-constrictor influence accompanies voluntary acceleration.

4
3
2
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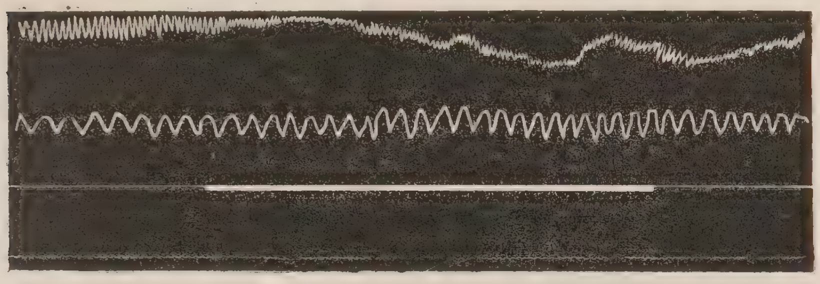

FIG. I. (from Professor Tarchanoff's article). — 1. Chronograph, one-second interval. 2. Period of acceleration. 3. Thoracic pneumograph. 4. Plethysmograph. To be read from left to right.

The acceleration of the beat in the above figure is at the rate of 27 a minute. There is also an increase of three respiratory curves a minute; but this slight increase in the number of respirations is quite insufficient to account for the great increase in the number of heart-beats. Salomé's vaso-motor system was very sensitive: even slight sounds in the room would increase his blood-pressure. He had control over his ear muscles, platysma myoides, and several other muscles which are not usually under the control of the will. Tarchanoff thinks that Salomé gets his power of voluntary control from an unusual organization of the nervous system. He believes there is a direct connection between the highest will centres of the hemispheres and the accelerating heart centres, which are situated in the upper cervical part of the spinal cord. He thinks the vagus plays no part in this acceleration. Tarchanoff says that after these experiments on Salomé he met several persons who had the power to accelerate their heart's beat; but all, without exception, had also a certain amount of control over muscles which were ordinarily beyond the voluntary control of man, such as the platysma myoides, muscles of the ear, and of the three terminal phalanges of the fingers. He had not met a single person having the usual control of the muscles who could influence his heart's beat.

By chance I heard that a member of the Harvard Medical School had a certain amount of voluntary control over his heart. This man, on learning I had chosen the subject of "Voluntary Control of Heart" to write on, offered to allow me to try some experiments upon him to test scientifically his power. As I knew practically nothing about the delicate apparatus used in experiments of this sort, Dr. H. P. Bowditch kindly conducted the experiments, the results of which I now present to you.

In these experiments a sphygmograph, pneumograph, chronograph marking two-second intervals, and an instrument to mark the period of acceleration, were used. Our subject, like Salomé, was unable to continue the stimulus for acceleration of the heart indefinitely, so in the following experiments I have reduced the actual figures to the basis of a minute. As the thoracic respiratory curve in our first experiments, as shown by the pneumograph, was rather small, a second pneumograph was placed over the subject's abdomen. The respiratory curve produced by this last instrument was much better marked. By means of these two

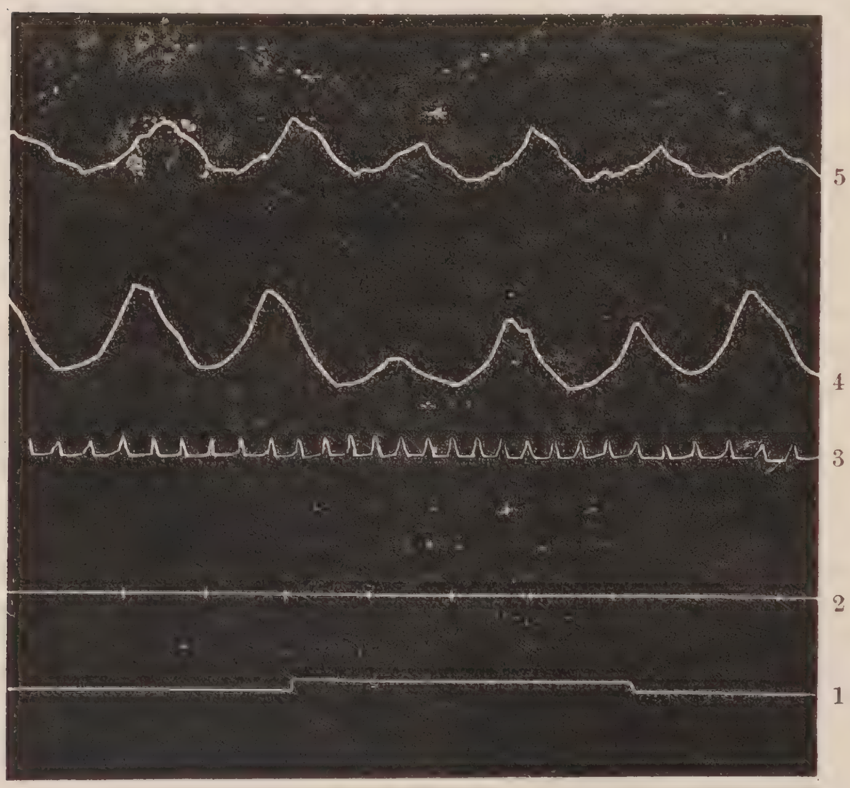

FIG. II.—1. Period of acceleration. 2. Chronograph, two-second intervals. 3. Sphygmograph on radial artery. 4. Abdominal pneumograph. 5. Thoracic pneumograph. To be read from right to left. An acceleration at the rate of 17 beats per minute.

instruments we could detect in the remaining experiments any slight change in the breathing.

Now the first figure I have for your inspection is one showing a great acceleration of the heart's beat with a fairly normal respiration. (Fig. ii. p. 6.)

This figure and also the following figures should be read from right to left. Tracing 1 marks the acceleration period by making a somewhat higher line during the effort. Tracing 2 is made by a chronograph marking two-second intervals. Tracing 3 is that of the transmission sphygmograph placed upon the radial artery of the right hand. Tracing 4 is that of the abdominal pneumograph. Tracing 5 is that of the thoracic pneumograph. The descending curves in the last two mark inspiration; the ascending curves, expiration. Now, in order to find out the acceleration I counted the heart-beats on the sphygmographic tracing between perpendicular lines drawn from the chronographic dots nearest the beginning and end of the acceleration period (having made corrections for any lack in perpendicularity there might have been in the position of the tracing pens), and then subtracted from the result thus obtained the number of heart-beats in the same length of time preceding the point where the stimulus for acceleration was put on. For the 8 seconds *before* acceleration there were 10.4 heart-beats. For the 8 seconds *during* acceleration there were 12.7 heart-beats. The acceleration in this case was 2.3 beats in 8 seconds, which is at the rate of $17\frac{1}{4}$ beats in one minute. The beat after acceleration does not return immediately, but gradually, to normal; as I found by counting the number of beats for the same length of time after the acceleratory effort had ceased to be called into action. Throughout the experiments I found that each acceleration was attended with a somewhat deeper inspiration, followed by a shortened expiration. This may be seen in tracing 4 in Fig. II. and to a less degree by tracing 5. The breathing becomes a trifle quicker during acceleration, but the increase is so slight that it is barely noticeable in the tracing. The question is whether the slight increase in rapidity and depth of respiration would cause an acceleration at the rate of over 17 beats a minute. This is not probable, as I will show later.

Fig. III. shows the result of an experiment in which the subject tried to accelerate his heart-beat as much as possible, without, however, any powerful

muscular contraction. (In the preceding experiments he had tried to breathe normally while accelerating.) The acceleration obtained was at the

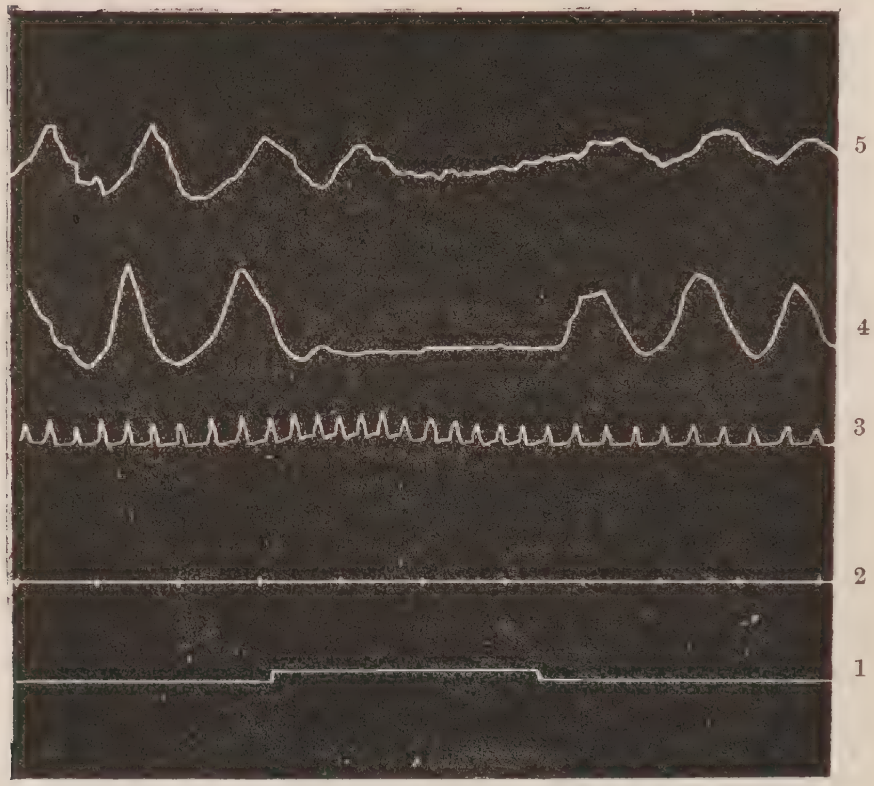

FIG. III. —1. Period of acceleration. 2. Chronograph, two-second intervals. 3. Sphygmograph on radial artery. 4. Abdominal pneumograph. 5. Thoracic pneumograph. To be read from right to left. Acceleration at the rate of 27 per minute.

rate of 27 a minute, the beat going from 79 to 106.

In order to get this great acceleration he held his breath at the middle of the thoracic respiration (as may be seen by referring to the figure), and then called up the stimulus to accelerate. It is to be noticed in this figure that there is a distinct rise of blood-pressure accompanying the acceleration of the heart-beat as seen by the general rise of the sphygmographic tracing. The increased blood-pressure appears soon after the effort at acceleration has begun, and lasts for some little time after the effort has ceased. The rise is better marked here than in the preceding or following figure. From the fact that his respiration stopped when the acceleration was most marked, it seemed possible that holding his breath might be the main cause of the great acceleration. Therefore an experiment was tried where the subject held his respiration until a tracing of the pulse was obtained when the heart was

under this condition, and then the word was given to accelerate.

The result of this experiment is seen in Fig. IV. The period of acceleration was six seconds. The normal beat was at the rate of 77 a minute; with checked respiration the beat was at the rate of 73 a minute; during acceleration the beat was at the rate of 87 a minute,—an acceleration, after the diminution caused by checked respiration, at the rate of 14 a minute. In another experiment similar to the one above the subject caused a still greater acceleration of his heart-beat, the sphygmograph registering an increase at the rate of 19½ beats a minute. These facts eliminate the possibility that

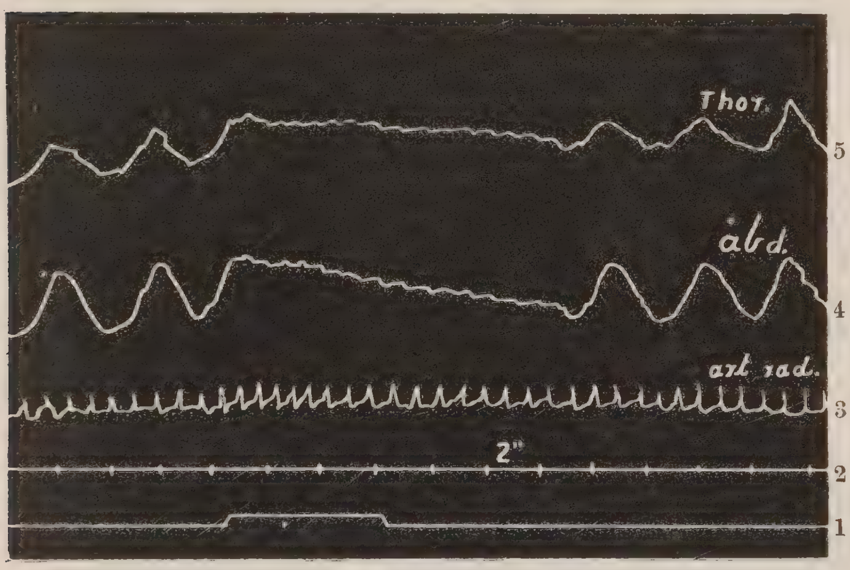

FIG. IV.—1. Period of acceleration. 2. Chronograph, two-second intervals. 3. Sphygmograph on radial artery. 4. Abdominal pneumograph. 5. Thoracic pneumograph. To be read from right to left. An acceleration of 14 per minute.

the acceleration was in any way due to a checking of the respiration, for, in this subject, holding the breath caused a very marked decrease instead of an increase in the number of heart-beats per minute.

In speaking of Fig. II. in these experiments I left for later consideration the fact that, although the subject endeavored to keep his breathing normal, there was with each acceleration a slightly deeper and quicker respiration. Now, in order to prove that the acceleration was not due to any increase in frequency or depth of breathing, the subject was asked to take several deep and rather quick respirations, without putting on the acceleration stimulus. In comparing the tracings of sev-

eral periods of normal respirations with several periods of these deeper and rather more rapid respirations I found that the moderately fast and deep breathing caused but very little increase in the rate of the heart-beat, — an increase that was totally insufficient to be considered the cause of the great acceleration obtained by this subject.

In the above remarks I have taken care to say periods of respiration (meaning thereby a certain number of full respirations), for Wertheimer and Meyer[13] say there is an acceleration of the heart-beat at each inspiration and a diminution at each expiration. As there were one or more full respirations in the cases I have presented to you my results are not dependent on any acceleration that may be found in a single inspiration. While bringing the Fig. II. to your attention I asked you to notice the marked rise in blood-pressure during acceleration as shown by the general rise of the sphygmographic tracing. The cause of this rise in blood-pressure is shown in the plethysmographic tracing in Fig. V. The plethysmograph used for this tracing was a small rubber tube fitting closely over the third finger and having between it and the end of the finger an air space delicately connected with a Marey drum.

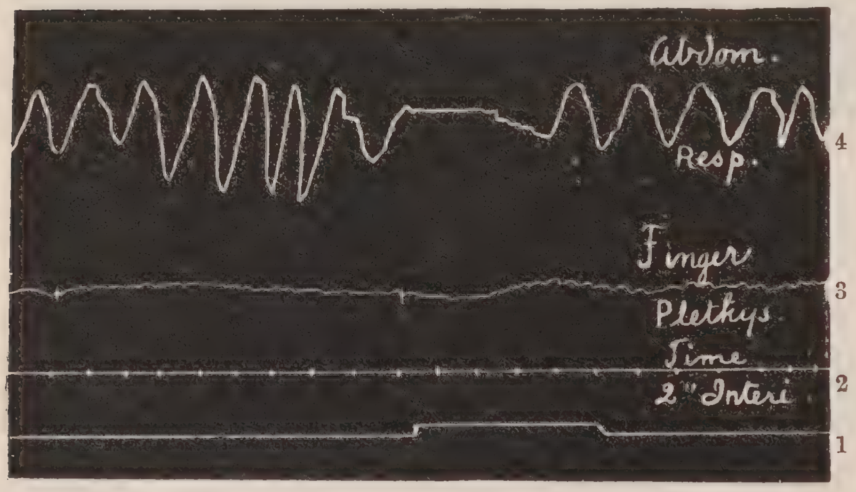

FIG. V.—1. Acceleration period. 2. Chronograph, two-second intervals. 3. Finger plethysmograph, on third finger. 4. Abdominal respiration. To be read from right to left.

The general fall in the plethysmographic tracing, appearing soon after the acceleratory stimulus, is caused by the diminution in size of the extremity,

[13] Archives de Physiologie, p. 24.

and indicates an increased blood-pressure due to a peripheral vaso-constrictor influence, accompanying the increased heart-beat. This result accords with Foster's[14] general statement that "variations in the heart-beat must always be looked upon as a far less important factor of blood-pressure than variations in the peripheral resistance." The tracing marked the greatest contraction of the organ, *i.e.*, the highest blood-pressure, very near the end of the acceleration period. There was then a secondary rise in the tracing similar to the one which Tarchanoff got in his experiments on Salomé.

Foster[15] also says that high blood-pressure is attended, contrary to expectation, by a slow heart-beat; and Marey states that the rate of the heart-beat is in inverse proportion to the arterial pressure, a rise in pressure being accompanied by a diminution in the pulse-rate. This fact is shown in the sphygmographic tracing on Fig. IV., where a slight rise in blood-pressure appears on holding the breath, and there also appears a corresponding decrease in the number of heart-beats until the stimulus for acceleration was put on, as I said while speaking of that figure. Since, then, a decrease in the number of heart-beats appeared with the rise in blood-pressure, it must be admitted that increased blood-pressure could not have been instrumental in bringing about this acceleration of the heart.

It is well to state here, perhaps, that the subject is a healthy, vigorous man who has never had any trouble with his heart. He has a certain amount of control over his ear muscles and platysma myoides. With each effort at acceleration of the heart he feels a sort of tingling reaching from the cerebral region to the heart and then spreading out toward the periphery. The accelerating impulse is not a single continued one but a series of short efforts gradually growing weaker. He could accelerate most when he felt strongest. His accelerating power decreases with every experiment, until finally he feels himself quite unable to produce further acceleration. After each experiment the perspiration would stand out in great drops on the palms of his hands, thus showing an intimate relation in the medulla oblongata between the cardiac nerves and the vaso-motor system. A slight head-

[14] Foster's Physiology, Third American Edition, Reichert, 1885, p. 242.
[15] *Loc. cit.*, p. 240.

ache followed the experiments, if they were somewhat prolonged. The subject is apparently unable to decrease in the least the beat of his heart.

From what I have said and shown to you I have come to the conclusion that the power of this subject to accelerate his heart-beat was not due to the calling up to his mind of any emotions or ideas which had a tendency to change his heart-beat (the word of the subject must of necessity be taken for this, as no experiment can prove it), nor to any movement of the regular voluntary muscles, nor to a change in breathing or blood-pressure, but was due simply and purely to an effort of the will directed upon the regulating mechanism of the heart.[16]

I here append, as a caution to those who are tempted to investigate for themselves this interesting subject, a translation of a letter which Prof. Tarchanoff wrote to the editor of *Die Archives für die Gesammte Physiologie* a short time after the appearance of his article: —

"It was very pleasant for me to learn that my work on the voluntary acceleration of the heart in man has been of such lively interest to you. This peculiar power is indeed remarkable, but unfortunately is very dangerous for those persons who have this gift, as I was unfortunately convinced in the case of Dr. Schlesinger. He could even double voluntarily the number of his heart-beats. But he now suffers from such a severe palpitation of the heart that he can no longer sleep quietly. Since I fear that the experiments made by me with Dr. Schlesinger may have caused these evil effects, I would ask other investigators to exercise the greatest caution in repeating my experiments.

[16] The figures used in this article are reproductions of photographs taken from the original tracings.

Extracted from the
American Journal of the Medical Sciences for November, 1889.

HOW FAR MAY A COW BE TUBERCULOUS BEFORE HER MILK BECOMES DANGEROUS AS AN ARTICLE OF FOOD?[1]

By Harold C. Ernst, A.M., M.D.,
of Boston.

The change of opinion in regard to the infectious nature of tuberculosis has been very marked in the last few years, not among the scientists, but among the people at large. Of course the medical world has, as a rule, accepted the conclusions to be drawn from Villemin's work of twenty-five years ago, and the discovery of the specific cause of the disease by Koch has only added strength to the theories advanced in certain quarters before that time.

The change of opinion spoken of is, after all, hardly a change, but, more properly, an acceptance of the knowledge gained in regard to the disease by the more recent and exact methods of research, and a much wider diffusion of that knowledge. More and more is it the rule that the knowledge of the transmissibility of tuberculosis by means of infected material is recognized among those whom it concerns the most, and nothing but good can come from the diffusion of that knowledge.

It is hardly too much to say that proper methods of management of tuberculosis, both in human beings and in animals, involve more important interests—pecuniary as well as vital — than any other subject that engages the attention of medical men. It is well known that one-seventh of the human race, approximately, perish from this disease, and when we acknowledge to ourselves, as a fair review of the evidence at hand must force us to do, that most, if not all, of this loss is preventable our duty is plain before us. That is, never to cease speaking of it, never to give up trying to reconcile the money interests of man with his own welfare, and to do all in our power, by the collection of clinical and experimental evidence, to make the case complete.

The work showing the etiological relationship of the bacillus of tuberculosis to the disease was, to all intents and purposes, complete upon the

[1] Read before the Association of American Physicians, Washington, Sept. 20, 1889.

publication of Koch's monograph upon the subject. Nothing more in the way of proof was actually needed, and, indeed, very little has been furnished. At the same time, confirmatory evidence was demanded by some who had and many who had not access to the original details, and this confirmatory evidence has been furnished in such overwhelming amount that it is to-day but a waste of time to repeat, what is accepted the scientific world over, that in the organism described by Koch we have the specific cause of this pathological change, and that without its activity we do not have tuberculosis in any form or under any conditions.

An imperfect understanding of the nature of bacteria in general, and of this organism in particular, has led to many attempts to arrest the pulmonary form of the disease it produces, by therapeutic measures, most of which would have been seen to be useless at the outset, if a knowledge of the problem had been complete. It is not upon drugs or mechanical means that our reliance should be placed in attempting to stamp out this scourge of civilized man. Our attention must be turned in the direction of proper preventive measures, and until the necessity for this is impressed upon physicians in general, and by them upon the people at large, so that the preventive measures suggested after mature deliberation will be complied with, but little can be effected, and the knowledge gathered after so much hard labor must be considered as wasted, for the time being.

In order to the suggestions upon which the stamping out of tuberculosis must depend, there is necessary a large amount of investigation into the methods by which it spreads and by which the virus is carried from person to person. Among these methods are undoubtedly the excreta—more especially the sputum—from persons affected with the disease; the excreta are carelessly treated and scattered broadcast to the injury of persons susceptible but not previously affected. The methods of distribution in this way, and the behaviour of the bacillus of tuberculosis outside of the body, have been well and recently treated by Cornet (*Zeit. f. Hyg.*, Bd. v. S. 191, 1888).

Other methods of distribution are of importance, however, and until within a few years have not received attention from the medical profession at all commensurate with their value. These methods of infection are those arising from the ingestion of food materials coming from the domestic animals, especially the flesh and milk of cattle.

In Koch's *Etiology of Tuberculosis* he uses the following expressions:

"Since by far the greatest number of cases of tuberculosis begin in the lungs, it is to be supposed that the infection in all these cases has taken place in the manner just suggested—by the inhalation of phthisic sputum dried and made into dust. The second principal source for the tubercle-bacilli, viz., tuberculosis of the domestic animals, appears not to have anything like the importance of the phthisic sputum. The animals, as is well known, produce no sputum, so that during their life no tubercle-bacilli get from them

into the outer world by means of the respiratory passages. Also in the excrement of tuberculous animals the bacilli appear to be only exceptionally present. On the contrary, it is a fact that the milk of tuberculous animals can cause infection.

"With the exception of this one way, therefore (*i. e.*, through milk), the tuberculous virus can only have effect after the death of the animal, and can only cause infection by the eating of the meat. The same conditions hold for the milk of cows suffering from 'perlsucht.' Before all things, if infection is to take place, it is necessary that the milk contain tubercle-bacilli; but this appears to be the case only when the milk-glands themselves are affected with the disease. This explains at once the contradictions in the statements of various authors, who have made feeding experiments with the milk from cows suffering from 'perlsucht.' If infection from tuberculous animals does not appear to be frequent, it must by no means be underrated."

This caution is one which was necessary at the time it was written, and its repetition is as necessary now as ever. What conclusions may be reached in regard to its *extreme* importance, are well shown by the statistics collected and presented by Dr. Brush before the New York Academy of Medicine, on April 29, 1889 (*Boston Med. and Surg. Journal*, cxx. p. 467 *et seq.*). In this paper the author states that after several years of close study of the affection, including a consideration of all accessible statistics, and the habits of the people among whom it prevails, he has arrived at the conclusion that the only constantly associated factor is found in the in-bred bovine species. If a community was closely connected with in-bred dairy cattle, tuberculosis prevailed, and, *vice versâ*, if there were no in-bred dairy cattle there was no tuberculosis. In the discussion following this paper many objections were raised. Dr. Brush went on to say that he believed that the disease was originally derived from the bovine species. He did not believe that less than fifty per cent of all dairy cattle were affected with it, while the statistics he had quoted showed that wherever there was a race of people without cattle phthisis was unknown. He believed, furthermore, that if all the cattle in this country were to be killed, the disease would finally die out entirely here.

Such statements as these are a revelation to the generality of practitioners, and may seem to be somewhat forced, but they certainly indicate, together with the statistics upon which they are based, the existence of a greater danger than has been fairly realized. That the danger from the consumption of milk coming from cows affected with tuberculosis has been understood by individuals at least, and that, too, before the announcement of Koch's discovery, is very well shown by extracts from a letter which I take the liberty of quoting here. The gentleman writing it is a veterinarian in practice in Providence, R. I., and the observations were made and the advice given more than ten years ago. That portion of his letter bearing upon the subject in hand is as follows:

"Mr. W., June 15, 1878, called me to see a white and red cow. Coughs and is short of breath and wheezes. Pulse 60; respiration 14, and heavy at

the flanks; temperature 104°. Diminished resonance of right lung, but increased in part of the same. Emphysematous crackling over left lung and dulness on percussion. Diagnosed a case of tuberculosis and advised the destruction of the animal.

"*Dec.* 12. Cow in a cold rain a few days ago for about two hours. Milk still more diminished than at a visit made on September 25th. Again advised the destruction of the cow. Family still using the milk. Respiration 20; pulse 85; temperature 104.6°.

"*Feb* 22, 1879. Temperature 104.8°; respiration 26; pulse 68. Losing flesh fast. Milk still in small quantities. Advised, as before, to destroy the animal and *not to use milk.*

"*May* 30. Called in a hurry to see cow. Is now as poor as could be. No milk for a week. Pulse 80; respiration 40; temperature 106°. The cow died in about three hours. Autopsy made fourteen hours after death: Lungs infiltrated with tuberculous deposit. Weight of thoracic viscera 43.5 pounds. Tuberculous deposits found in the mediastinum, in the muscular tissues, and in the mesentery, spleen, kidneys, udder, intestines, pleura, and one deposit on the tongue. The inside of the trachea was covered with small tubercles.

"In August, 1879, the baby was taken sick, and died in about seven weeks. On post-mortem of the child there was found meningeal tuberculosis—deposits all over the coverings of the brain and some in the lung.

"In 1881 a child, about three years old, died with, as it was called, tuberculous bronchitis. And in 1886, a boy, nine years old, who for three or four years had been delicate, died with consumption—'quick,' as it was called.

"So far as known, the family on both sides have never before had any trouble of the kind, and the parents were both rugged and healthy people, and so were the grandparents—one now being alive and sixty-eight years old, and the other dead at seventy-eight."

Of course there is much room for criticism, if these cases be quoted as carrying out an exact clinical experiment, and no one can say that the occurrence of the three deaths in the same family was anything more than a coincidence. At the same time it must be acknowledged that they offer very solid suggestions for consideration, and that the light thrown upon the disease by the investigations of recent years make the advice of the veterinarian to "kill the cow and stop using the milk" much more sound than it appeared to the minds of the medical gentlemen who "laughed" at him at the time it was given.

It is my hope within the coming year to collect a series of clinical observations which will be of interest and some service in elucidating the question of how many cases of tuberculosis occur which produce suspicion in the minds of medical or veterinary attendants of having an origin in the milk from infectious cows.

It is upon this question of possible danger from the domestic animals —especially cattle—that much recent work has been done, but the subject has been by no means exhausted.

If there is danger to human beings from the widespread existence of tuberculosis among cattle, some sort of restrictive measures must be taken, by means of which this danger can be lessened. At the same time legislation calling for so much pecuniary loss as would be the case if the present supply of tuberculous cattle were to be destroyed, can

only be asked for with a backing of as much carefully gathered scientific evidence as can be obtained, and it is the part of preventive medicine and the experimental method to furnish some of this evidence.

Through the liberality and broad-mindedness of an association of gentlemen in Boston, it is possible to present the results of certain experiments undertaken to determine the question which is expressed in the title of this paper. "How far may a cow be tuberculous before her milk becomes dangerous as an article of food?" is an extremely important point to decide. If it be considered already settled and Koch's dictum be accepted, that there is no danger in the milk, if the mammary glands be not affected, then there remains only for the veterinary surgeon to determine the existence of such lesions, and restrictive measures can go no further. If, however, the milk from cows with no visible lesion of the lacteal tract be shown to contain the specific virus of the disease in a not inconsiderable number of cases, and if this milk be shown to possess the power of producing the tuberculous process upon inoculation in small quantities and in feeding experiments carried out with every possible precaution, then restrictive measures must have a far wider scope, and be carried on from an entirely different standpoint than has heretofore been considered necessary.

It is familiar to most of us that little importance has been attached to this question—the danger of milk from tuberculous cows with no lesions of the udder—for the reason that many experiments have been made with negative results, and because *à priori* reasoning would seem to indicate the absence of such danger; because tuberculosis is not a disease like anthrax, in which the specific poison is to be found in all parts of the system and is carried from one place to another by the blood-stream. Koch's assertion that the milk from cows affected with tuberculosis is dangerous only when the udder is involved, appears to be based upon theoretical considerations rather than practical work in this especial direction. It has been widely accepted, however, and the weight of his name has caused the assertion to be repeated many times with but few attempts to verify its correctness.

The increased attention that has been paid to the disease among cattle, and the suspicions that have been aroused that tuberculosis among the domestic animals is a more frequent cause of its appearance among men than has been supposed, have made a careful investigation of this point imperatively necessary. With the exception of a few successful experiments by Bollinger (*Deutsch. Zeit. f. Thiermed.*, Bd. xiv. S. 264) and Bang (*Ibid.*, Bd. 11, S. 45, 1885), no evidence of great value is to be adduced. These authors, as well as Tschokke (quoted by Bollinger), bring out isolated cases showing successful inoculation experiments with the milk from tuberculous cows with no disease of the udders, but the experiments are so few in number that they cannot be

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accepted as furnishing more than a probability, and extremely critical persons might be justified in ascribing the results to contamination.

Bang (*Congrès pour l'étude de la Tuberculose*, 1, p. 70, 1888) gives new results. Examining twenty-one cases of cows affected with general tuberculosis but with no signs of disease in the udder, he found but two whose milk showed virulent qualities upon inoculation in rabbits. He concludes that since the cows experimented with were in advanced stages of the disease and yet showed such slight virulent properties in their milk, the danger from cows in less advanced stages is much less. And this conclusion he thinks is borne out by experiments with milk drawn from eight women affected with tuberculosis; specimens were used from all for inoculation and none were found to be virulent. He draws the conclusion, therefore, that it is not necessary to consider all milk dangerous coming from tuberculous cows, but that it should always be *suspected*, because no one can say when the udder will be diseased, and because, without this, the milk from tuberculous cows contains the virus in rare cases.

I shall endeavor to show that it is not at all rare for such milk to contain the virus.

Galtier also (*loc. cit.*, p. 81) has given the result of certain experiments with milk coming from tuberculous cows, but he says that

> "certain experimenters claim to have established the virulence of milk coming from animals whose udders appeared to be normal and free from any lesions; the greater number, and I am one of them, have merely encountered a virulence in milk after the udder had become tuberculous. However, as a beginning tuberculosis of the udder is an extremely difficult thing to recognize, especially during the life of the animal, the milk should be considered dangerous which comes from any animal affected, or suspected of being affected, with tuberculosis."

I shall endeavor to show that this view of the case is justified by something more than probabilities.

In the *Deutsch. Arch. für klin. Med.*, Bd. xliv. S. 500, Hirschberger reports the results of an experimental research upon the infectiousness of the milk of tuberculous cows, in which—following out Bollinger's work—he attempts to settle, 1st, whether the cases are rare in which tuberculous cows give an infectious milk; and 2d, whether the milk is infectious only in cows with general tuberculosis, or whether it is also infectious when the disease is localized. He made the trials of the infected milk by injection into the abdominal cavity of guinea-pigs with the usual precautions. His results were as follows:

1. Milk was used five times from cows affected with a very high degree of general tuberculosis in all the organs.

2. Milk was used six times from cows with only a moderate degree of disease.

3. Milk was used nine times from cows in which the disease was localized in the lung.

From these twenty cases the milk was proven to be infectious in eleven. The percentage of positive results in the animals when arranged in accordance with the three groups above given was 80 per cent. in the first group (milk from cows in a very advanced stage of the disease), 66 per cent. in the second group, and 33 per cent. in the third. He found the bacilli in only one of the specimens of the milk, and considers that this, therefore, shows that the inoculation experiments are the more certain guide as to whether the milk is infectious or not.

These results are extremely interesting, although they do not lay as much stress as do mine upon the presence or absence of lesions of the lacteal tract.

The experiments which I am able to report[1] have been made possible by the liberality of the Massachusetts Society for the Promotion of Agriculture, which became interested in the question some time ago, and has put it in my power to carry them on. They have given everything in the way of pecuniary and moral support that the work has required; my own part has been that of general director, and I have had associated with me during the whole time the Society's veterinarian, Austin Peters, D. V. S. For the last year I have also had the very valuable aid of Dr. Henry Jackson and Langdon Frothingham, M. D. V.

All of the inoculation experiments and most of the microscopic work have been done in the bacteriological laboratory of the Harvard Medical School, some of the microscopic work at the Society's laboratory in Boston, whilst the feeding experiments have been done and the experimental animals have been kept at a farm in the country devoted to this especial purpose, and situated among the healthiest possible surroundings. Nothing has been set down as the result of microscopic observation that I have not myself verified, and every portion of the work has been carried out under the most exacting conditions and with every possible precaution against contamination.

Before the farm buildings were used at all they were thoroughly cleaned from top to bottom. Every portion of old manure was carted away, as well as all the old earth. The whole of the woodwork was scrubbed and then washed with corrosive sublimate solution (1 : 1000) and finally whitewashed, and every care was taken to secure good drainage and free ventilation. The result and effectiveness of all this have been best demonstrated by the fact that every animal brought to the place made a most marked improvement in its general condition, while some of them even went so far as to appear to get well.

[1] The full notes of these experiments will be found in the Transactions of the Association of American Physicians, vol. iv., 1889.

In deciding whether the milk from any cow affected with tuberculosis is dangerous, when the udder shows no lesion, the first point is to see whether the milk contains the infectious principle or not. In this case, of course, that infectious principle is the bacillus of tuberculosis, and attention was turned to that for some time. The observations have been carried on over a long space of time, and were made as follows: The milk was taken from the cow in the morning—or evening, as the case might be—the udders and teats having just been thoroughly cleansed. The receptacle was an Erlenmeyer flask, stoppered with cotton-wool and thoroughly sterilized by heat. The specimen was taken at once to the laboratory, there placed n conical glasses, with ground-glass covers—the whole of these having been carefully cleansed beforehand—and then allowed to stand in a clean refrigerator for twenty-four to forty-eight hours, and sometimes for seventy-two hours.

At the end of that time from ten to twenty cover-glass preparations were made from various parts of the milk or cream. These were stained after Ehrlich's twenty-four hour method, with fuchsin and methylene blue as a contrast color, and then searched with an immersion lens.

We prepared for examination in the way spoken of above, one hundred and seventeen sets of cover-glasses from as many different samples of milk. Of these specimens three spoiled, *i. e.*, turned sour or acid before the examination was completed, and must be rejected, leaving, therefore, one hundred and fourteen samples of milk of which the examination was completed. These samples were obtained from thirty-six different cows, all of them presenting more or less distinct signs of tuberculosis of the lungs or elsewhere, but none of them having marked signs of disease of the udder of any kind.

Of these samples of milk there were found *seventeen* in which the bacilli of tuberculosis were distinctly present; that is to say, the *actual virus was seen in* 31.5 per cent. of the samples examined (36 : 114 =31.5). These seventeen samples of infectious milk came from ten different cows, showing a percentage of *detected* infectiousness of 27.7 per cent. (10 : 36=27.7). These results are exceedingly interesting, it seems to me, and I confess I am surprised at the size of the percentage named. Not because I had not expected to find the bacilli—I have been convinced for several years that persistent search would show their presence in such cases as those that are here recorded—but because the amount of dilution to which the organisms must be subjected diminished immensely the chance of their being found at all. In no case have they been seen in large numbers, but equally in no case has a diagnosis been made where there was the slightest doubt of the appearances under the microscope.

The large number of cases in which these organisms have been found seem to me to indicate their presence in a still greater proportion of

cases, if only a sufficiently thorough examination of all the milk could be made. This of course is out of the question, but the results here given seem to establish, beyond a doubt, the fact that milk coming from cows with no definite lesion of the udder may contain the infectious principle of tuberculosis, if the disease be present in other portions of the body of the animal. Also, that this presence of the infectious principle is not merely a scientific *possibility* but an actual *probability*, which we should be thoroughly aware of and alive to.

Other interesting facts shown are these: that the cream after rising is quite as likely to be infectious as the milk, because the bacilli were found in the milk nine times after the cream had risen, and in the cream eight times after it had separated from the milk.

In regard to the constancy of the occurrence of the bacilli in the milk, in two of the ten cows in whose milk the bacilli were found, but one sample of the milk was examined; and the bacilli were found in one sample out of several examined at different times, in two cases. In the remaining six cows, bacilli were found two or more times in different samples of the milk. So that, as far as they go, these results seem to indicate that the bacilli are present with a fair degree of constancy. At the same time it should not be surprising if one examination was successful and others failed, because of the chances against success, owing to dilution, which were spoken of above.

In nine of the seventeen cases the time of the milking and the portion of the milk used were noted; that is to say, a sample was taken from the first of the milking, or the last of the milking, and then cover-glasses made from the milk or cream. In these cases bacilli were found in the cream three times, and in the milk four times, from the first of the milking; in samples from the last of the milking, in the cream no times, and in the milk four times; and this too seems to show an interesting point, viz., that the bacilli, if present at all in the udder, are not washed out entirely by the first manipulations of the teats, but may be supposedly present in any portion of the milk. The converse is also indicated, that the manipulation of the udder in the process of milking does not express the bacilli from the tissue into the latter portion of the milk, but that, as before, they may be supposed to be pretty evenly distributed in all parts of the udder if they be present at all.

Before going on to consider the results of the inoculation experiments made with various specimens of milk, it may be well to glance at the condition of the cows that have been under our control from the time of the beginning of the experiments until they were killed, or until the date of preparing this paper.

The history of each cow, as far as we have been able to secure it, bears out our assertion—as far as the examinations have gone, that none of the udders were affected with tuberculosis—certainly so far as gross

appearances were concerned. This was true, also, in the microscopic appearance of every case but one (No. 6, cow F). In this case the gross appearances in the udder were healthy, except that one quarter seemed to be slightly fibrous, and there were one or two yellow spots which were seen to be made up of fat under the microscope. With a low power lens only a slight increase of fibrous tissue was observable, and the oil-immersion was put on merely as a matter of routine. One giant cell was discovered containing a number of bacilli, but a careful search failed to show any others, or any signs of change, except the increase of fibrous tissue noted above. So that the assertion is still true, that we have failed to discover any signs of tuberculosis that were easily recognizable in any of the cows here recorded, and these include all we have had under closer observation.

Those from which milk was used for inoculations that are not here given had no signs that permitted of even a probable diagnosis by skilled veterinarians.

We also made an interesting series of experimental inoculations in rabbits and guinea-pigs with milk or cream from various cows, in varying quantities and at different times. Of rabbits there were used fifty-seven animals. Of these, five were inoculated with milk which had turned sour, two died of intercurrent diseases in a few days (coccidium oviforme), and of one the material was lost before the microscopic examination was completed—so that eight animals are to be rejected, leaving forty-nine upon which the results can be based. Out of these, five were made more or less tuberculous, as proven by microscopic examination, and in forty-four the results were negative—that is to say, we obtained 5 : 49, or 10.2 per cent. of successes out of all inoculations in rabbits.

There were used thirty-three different specimens from thirteen different cows—that is, there were 23 per cent. (3 : 13) successful results from the cows used, and 15.15 per cent. (5 : 33) successful results from the specimens used.

Positive results were obtained from

Cow P twice (at different times).

Cow L once.

Saunders cow twice (at different times).

The results of the inoculations of guinea-pigs are more striking. There were sixty-five animals used in all. Of these, nine were inoculated with sour milk or cream, and two died in a day or two of other diseases (peritonitis and pleurisy). There are, therefore, but fifty-four that should be counted. In them, there were twelve positive results, or 28.57 per cent. (12 : 42) successes out of all the inoculations. There were used thirty-two specimens from fourteen different cows, and the successful results came from six different cows—that is, 42.8 per cent. (6 : 14) of the cows were shown in this way to have infectious milk, and 37.5 per

cent. (12 : 32) of the specimens used were shown to have active infectious properties.

Positive results were obtained from

Cow P (three times in two different inoculations).

Cow D (three times in three different inoculations).

Cow F (once).

Slocum cow (once).

Saunders cow (once).

Mayhew cow (three times in two different inoculations).

The combining of the results obtained from both rabbits and guinea-pigs shows the following: Successful results were obtained in milk from cow P three times (two different specimens) in guinea-pigs, and twice in rabbits (two different specimens); from cow L once in rabbits; from cow O three times (three different specimens) in guinea-pigs; from cow F once in guinea-pigs; from the Slocum cow once in guinea-pigs; from the Saunders cow once in guinea-pigs, and twice in rabbits (two different specimens); and from the Mayhew cow three times in guinea-pigs (two different specimens)—that is to say, out of fourteen cows used the milk was shown to be infectious in seven, or 50 per cent., by inoculation experiments.

An interesting fact is also shown, and that is, that bacilli were found in the milk or cream, and successful inoculation experiments made in animals with the same specimen in five different cases (including eight of the successful ones) as follows:

Comparison of the dates when Bacilli were found in the Milk and the same Milk was used for successful inoculation experiments.

Cow.	*Positive.* Cover-glass.	*Positive.* Guinea-pig.	*Positive.* Rabbit.
P.	Cream, A. M.	Cream, A. M., March 9, 1889	Cream, A. M., March 9, 1889
	Cream, P. M.	Cream, P. M., March 9, 1889 Cream, P. M., March 9, 1889	Cream, P. M., March 9, 1889
O.	First of milking, cream, March 9, 1889.	First of milking, cream, March 9, 1889	
Slocum.	Last of milking.	Last of milking, June 10, 1889	
Mayhew.	Last of milking, milk.	Last of milking, milk, June 21, 1889	

The inoculation experiments, above detailed, seem to me to be deserving of consideration because they were done under the most careful precautions that could be devised. In all cases the experiment animals were kept under observation long enough to determine, so far as could be seen, that they were in good health, and after the inoculations they were separated and kept under close watch, but in healthy surroundings. Some of those that were used were inoculated immediately after purchase, because of a scarcity of the supply at the farm, and were not in good condition. But as no sign of tuberculosis appeared in any of these, their ill health cannot come in as a disturbing factor in the results.

The results obtained from certain feeeding experiments with calves show that there were thirteen calves used, and fed for varying lengths of time with milk from cows affected with tuberculosis, but not of the udder. Of these, the material was thrown away from one before the microscopic examination, and this should be rejected in the final results. Of the remainder there were five positive results obtained and one suspicious. The latter is counted as negative, for the reason that, although giant cells and granulation tissue were seen, no bacilli were found. There were, therefore, five out of twelve positive results, or 41.66 per cent. It should also be said that of those counted as negative three sets of specimens were suspicious, but were hastily examined for the purposes of this paper, so that a more careful search may very probably increase the percentage of successes.

In the series of feeding experiments on one set of pigs, the milk being given to them from the same cows as before, there were seven pigs used in all, from one litter and healthy parents. Of these, examination showed negative results in two, positive results in two, one was subjected to a very hasty microscopic examination, and the material from two was thrown away—a mistake, as was shown by the results of the microscopic examination of the material from No. 3. There are to be counted, therefore, only five, giving as successful results 40 per cent.

By the cover-glass examinations we have shown that the milk contains infectious material in ten cows out of thirty-five from which the milk was examined for bacilli—that is, in 28.57 per cent. We have also shown that the milk was infectious, by inoculation experiments, in seven out of fourteen of the cows from which the milk came—that is, 50 per cent. And we have shown the infectious nature of the milk by ocular demonstration and successful inoculation from the same specimens in five cows out of fourteen used—or, 35.7 per cent.

These results are, to a certain extent, preliminary—that is to say, they are but part of the work upon this subject which is being done under the auspices of the Massachusetts Society for the Promotion of Agriculture. The work will not be completed, at any rate, until next year.

They show, however:

1st, and emphatically, that the milk from cows affected with tuberculosis in any part of the body may contain the virus of the disease.

2d. That the virus is present whether there is disease of the udder or not.

3d. That there is no ground for the assertion that there must be a lesion of the udder before the milk can contain the infection of tuberculosis.

4th. That, on the contrary, the bacilli of tuberculosis are present and active in a very large proportion of cases in the milk of cows affected with tuberculosis but with no discoverable lesion of the udder.

Ueber den Nachweis der Unermüdlichkeit des Säugethiernerven.

Von

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Als Bernstein[1] den Froschnerven näher dem centralen Ende reizte und näher zur Peripherie elektrotonisirte, und damit den Uebergang der Erregung auf den Muskel hinderte, gelang ihm der wichtige Nachweis, dass der Nerv später als der Muskel ermüdet. Hieran anknüpfend konnte Wedenski[2] unter Anwendung des gleichen, jedoch verbesserten Verfahrens darthun, dass der Nerv durch eine, viele Stunden hindurch fortgesetzte Tetanisirung nicht ermüdet. Gleiches mit Hülfe des Curare an dem Froschnerven nachzuweisen, gelang ihm nicht.

Angeregt von Wedenski fasste ich den Plan, den Beweis für die Dauerbarkeit der Nerven durch Curare zu erbringen.[3] Dieser einfache Weg führte zum Ziel, als an die Stelle des Frosches die Katze trat. Das Thier wurde mit Aether betäubt, einerseits der N. ischiadicus durchschnitten, das freie Ende des peripheren Stumpfes in Elektroden gelegt und der Abstand der Inductionsrollen aufgesucht, welcher der M. tibialis ant. in Tetanus versetzte. — Die Sehne des eben genannten Muskels war schon vorher freigelegt und auf passende Weise mit einem Hebel verbunden, dessen Ende sich an das berusste Papier einer sehr langsam und spiralig gedrehten Trommel lehnte. Nach diesen Vorbereitungen wurde das Thier

[1] Pflüger's *Archiv* u. s. w. 1887. Bd. XV.

[2] *Centralblatt für die medicinischen Wissenschaften.* 1884.

[3] *Journal of physiology.* 1885. t. VI; — Siehe auch Maschek, *Sitzungsberichte der Wiener Akademie.* 1887. Dritte Abthlg.

mit Curare vergiftet, sogleich der Nerv mit den wirksamen Inductionsströmen verbunden und mit der Reizung ununterbrochen fortgefahren.

Anderthalb bis zwei Stunden nach seiner einmaligen Anwendung, vier bis fünf Stunden nach wiederholter lässt die Wirkung des Giftes ganz allmählich nach, ein oder der andere Muskel regt sich leise ohne sichtbare äussere Veranlassung und nahezu gleichzeitig macht sich auch der Inductionsreiz auf den M. tibialis geltend, jedoch nicht so, wie es nach der Art des Reizes zu erwarten war. Statt des Tetanus, den das Inductorium vor der Vergiftung einleitete, erscheinen zunächst nur einzelne Zuckungen, zwischen welchen längere Zeiten der Ruhe eingeschoben sind, und erst später bringt es die Reizung zu einem unregelmässigen Tetanus.

Ob die eigenthümlichen Folgen der Reizung im Beginn der Entgiftung bedingt seien von einer gewissen Ermüdungsstufe der Nerven, veranlasst von der stundenlang fortgesetzten Reizung, oder ob sie auf Eigenthümlichkeiten zurückzuführen seien, die den Nerven und Muskeln bei der allmählichen Entfernung des Curare aus dem Organismus zukommen, schien mir einer erneuten Untersuchung werth.

Zeit und Gelegenheit zu ihrer Ausführung bot mir ein längerer Aufenthalt in Deutschland und das Entgegenkommen meines Freundes C. Ludwig. In Leipzig mussten in Ermangelung der früher angewendeten Thiere nur Hunde dem Versuche unterworfen werden. Mit ihnen wurde gerade so verfahren, wie vordem mit den Katzen. — Angewiesen auf die Hülfsmittel des hiesigen Institutes, benutzte ich zur Befestigung des Unterschenkels die von Lukjanow[1] beschriebene Klemme, wodurch es unmöglich wurde, dass sich die Bewegungen anderer Glieder in der auf dem rotirenden Papier niedergeschriebenen Linie geltend machen konnten. In allen übrigen Punkten stimmte Vorbereitung und Ausführung des Versuches mit den zu Boston ausgeführten überein.

Das Ergebniss mehrfach wiederholter Versuche bestätigte die früher erhaltenen. Wenn die volle Wirkung des Curare, nachdem sie drei bis vier Stunden hindurch bestanden hatte, nachliess, was sich durch sog. willkürliche Bewegung dieses oder jenes Muskels offenbarte, so wirkten auch die Inductionsströme wieder, welche von der Einführung des Giftes an ununterbrochen den peripheren Stumpf des N. ischiadicus durchsetzt hatten. Die ersten Zeichen der wiederkehrenden Herrschaft des Nerven über den Muskel äusserten sich durch einzelne, zeitlich von einander getrennte Zuckungen. Was den rasch aufeinander folgenden Inductionsströmen im Beginn des Aufwachens aus der Vergiftung nicht gelang, wurde jedoch unter der fortdauernder Reizung erreicht bei weiterer Abnahme der Vergiftung. Statt der ein-

[1] *Dies Archiv.* 1886. Suppl.-Bd. S. 126.

zelnen Zuckungen kam es nun zu einem Tetanus, aber auch dieser zeigte, keinen gleichmässigen, vielmehr einen sehr ungleichen Verlauf. Als Beispiel dafür, was der Muskel aufzeichnet, diene die Fig. 1.

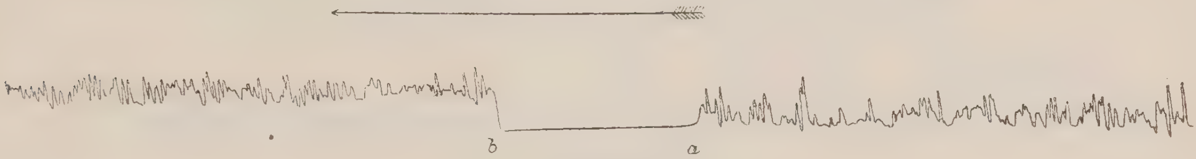

Fig. 1.

Durch diese Versuche hatte sich gezeigt, dass der Hund geeignet sei, um an ihm die oben aufgeworfene Frage zu entscheiden: ob der durch das Curare hervorgerufene Zustand oder ob die anhaltende Reizung des Nerven anzuklagen sei, wenn durch die gleichmässig wirkenden Inductionsströme ungleichmässige Zusammenziehungen des Muskels veranlasst werden.

Den gesuchten Aufschluss gewährten Thiere, welche mit Curare vergiftet waren, deren Nerven aber nur sehr vorübergehend und namentlich bei beginnender und wieder verschwindender Wirkung des Giftes gereizt wurden.

Um die Leistungsfähigkeit des Nerven möglichst zu schonen und doch den Zeitpunkt des Wiedererwachens aus der Vergiftung nicht zu versäumen, wurde selten und dann nur während kurzer Zeit mit Inductionsströmen von geprüfter Wirksamkeit gereizt.

Wenn der Nerv nach längerer Dauer der vollen Vergiftung wieder zur Herrschaft über den Muskel gelangt, so geschieht dieses nicht sogleich im vollen Umfange, denn wenn nach der Periode ihrer Unwirksamkeit die Inductionsschläge den Muskel wieder zu erregen vermögen, bringen sie statt des erwarteten Tetanus nur Anfangszuckungen hervor. Ein Beispiel für die Folgen der Reizung während der verschwindenden Curarevergiftung giebt Fig. 2.

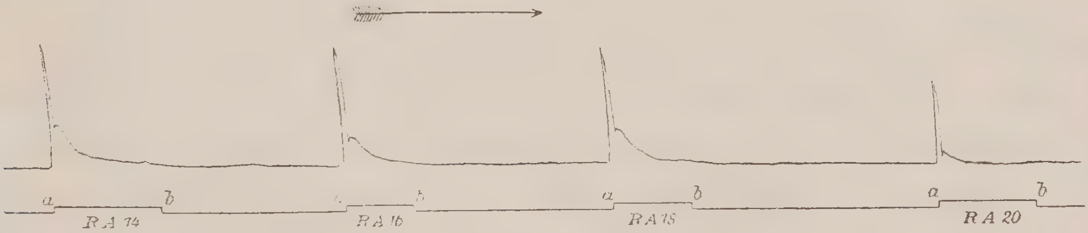

Fig. 2.

Auf der oberen Linie sind die Zuckungen, auf der unteren von je *a* zu *b* die zugehörigen Reizungsdauern und -Stärken geschrieben. — Mit dem Hereinbrechen der Inductionsströme verkürzt sich der Muskel rasch, um sich alsbald, wenn auch allmählicher zu verlängern. Wenn dann, nachdem sie einige Zeit hindurch unterbrochen gewesen, die Reizung von Neuem beginnt, so wiederholt sich, wie die Figur zeigt, die frühere Erscheinung.

Doch mit der fortschreitenden Zeit ändern sich die Folgen, welche die Inductionsströme erzielen; allmählich gelingt es ihnen, einen dauernden Tetanus zu erzielen, wie ihn die Fig. 3 darstellt. — Während seines Ver-

Fig. 3.

laufes zeigt er viele Unregelmässigkeiten, starke und schwache Verkürzungen wechseln unabhängig mit einander, doch erinnert sein Ansehen insoweit an das vorhergehende Stadium der Vergiftung, als auch der Anfangstheil des Tetanus umfangreichere Verkürzung aufweist.

Um zu erklären, warum sich während schwacher Vergiftung mit Curare der Nerv zum Muskel eigenthümlich verhält, dazu reichen die vorgelegten Beobachtungen nicht aus, wohl aber genügen sie zur Entscheidung der aufgeworfenen Frage. Sie beweisen, dass die Beantwortung einer Reihe von Inductionsströmen durch einzelne Zuckungen und unregelmässige Tetani dem besonderen Zustand zugeschrieben werden müsse, in welchen das Verbindungsstück von Muskel und Nerv während der fortschreitenden Entgiftung geräth. Denn es zeigte sich, dass die Folgen der Reizung zu der Zeit, in welcher die Wirkung des Giftes zu verschwinden begann, genau übereinstimmten, gleichgültig ob der Nerv bis dahin geruht hatte oder stundenlang von kräftigen Inductionsströmen durchsetzt worden war. — Damit ist der letzte Zweifel an der Befähigung des Säugethiernerven beseitigt, ohne Ermüdung kräftige und andauernde Reizung überstehen zu können.

Die Erfahrung, dass der Nerv, ohne zu ermüden, viele Stunden hindurch gereizt werden kann, lässt die Vorstellung aufkommen, dass die Erregung sich ohne jeglichen Verbrauch an Stoffen fortpflanzen könne. Zu der Annahme, dass das Fortschreiten der Erregung nur auf einer Verschiebung der Nervenmasse ohne irgend welche Zerlegung derselben beruhe, passen die Messungen Rolleston's.[1] Mit einem äusserst empfindlichen Bolometer konnte er keine Steigerung der Temperatur des Nerven, welcher anhaltend tetanisirt war, nachweisen, wohl aber eine solche, wenn der Nerv abstarb. Indessen stehen auch der Annahme, dass die Fortpflanzung der Erregung ein rein physikalischer Vorgang sei, Bedenken entgegen, z. B. die negative Schwankung. Aber wenn auch die Bewegung zu ihrem Fortschreiten durch den Nerven eines Kraftaufwandes bedürfte, welcher aus der Nervenmasse selbst bestritten werden müsste, so würde dieser doch von einer unmessbar geringen Grösse sein.

[1] *Journal of physiology.* t. XI. p. 208.

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THE CLOSURE OF THE CRANIAL SUTURES AS A SIGN OF AGE.[1]

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It is, I believe, pretty generally admitted among anatomists that the time and order of the closing of the cranial sutures are very uncertain, far too much so for them to be trustworthy guides to determine the age of the skull. Humphry's remark is well-known: "In some skulls edentulous from age, I have found them still well-marked; whereas in others, which bore evidence of having only just reached maturity, they could scarcely be traced." Nevertheless, it is still rather vaguely taught that they begin to close about middle life, or, according to some, from thirty to forty. Testut, in his new work on anatomy states that "la synostose physiologique débute vers l'age de quarante-cinq ans." It seems to me that medico-legal writers are, on the whole, rather more confident than the anatomists. Tidy writes: "In old age . . . the sutures become firmly ossified and generally less distinct. If the sutures of the skull are indistinct we may thus fix the age as at least between fifty and sixty." Some others say pretty much the same thing. There is, I think, little doubt that the statements of one writer on this point are simply repeated by the next with little attempt at verification. Topinard makes the following very precise statements in his "Anthropologie": "In fine, if no suture is affected, the person is about thirty-five years old or less. If the hind part of the sagittal is beginning to close he is about forty. If the coronal suture is ossified near the bregma he is fifty or more. The temporal suture being closed he is seventy or over." He admits, however, that the time of the closure of the sutures varies within large limits. I could mention a circular from a high official source in this country in which the following directions were given for estimating the age of bodies in which it could not otherwise be obtained. "This [the condition of the sutures] it is claimed by

[1] This is part of a paper entitled "Medico-Legal Studies on the Human Skeleton," read December 27, 1889, at the meeting of the Association of American Anatomists at Philadelphia. The number of heads examined was then 69. The observations have since been carried to 100, and the paper has been practically rewritten.

anthropologists, affords the most reliable guide to the determination of the age in such cases. If the sagittal or coronal suture shows bony union, the individual is said to be at or beyond middle life, or somewhere in the vicinity of forty-five years; if sagittal and coronal are joined, it indicates forty-five to fifty years; if sagittal, coronal and lambdoidal are synostosed, the age indicated is said to be from fifty-five to sixty-five; and if all the sutures are obliterated, the age is from sixty-five to eighty years."

With regard to the order of the closure of the sutures, Humphry teaches that "the fusion of the bones commonly takes place first at the fore part of the sagittal suture, next in the lambdoidal suture near the sagittal, then in the coronal suture near the inferior angle of the parietal bones." Sappey holds that it begins in the sagittal suture near the parietal foramina and travels both forward and backward, that the coronal begins to close at its lower ends, and the lambdoidal above. According to Merkel, the process usually begins in the sagittal, between the parietal foramina, and the coronal is usually the last to close. Rambaud et Renault gives the same order. The most thorough study of this subject is probably that by Sauvage "Sur l'état Sénile du Crâne,"[2] founded on tables of observations made by Broca at Bicêtre and at la Salpêtrière on 126 subjects of known ages. The most important conclusions concerning the three chief sutures are the following: the first to ossify is the sagittal, the union beginning behind in the part between the parietal foramina. Later, the process is more active in the middle than in the front part. The lambdoidal closes next. The process begins on the right, then is continued in the middle, and finally on the left. The closure is most complete in the middle. The coronal comes a little later; the middle part is the first to close, and is more completely obliterated than the lateral ones. The right side closes before the left. The obliteration always begins on the inside of the skull. It is very uncommon for the sutures to be obliterated on the outside of the skull. In general the obliteration begins towards forty-five years. So far Sauvage. Topinard gives the following somewhat different statements, but ends his remarks by referring to Sauvage in a foot-note. This first point of closure is usually in the posterior part (but not at the end) of the sagittal, but sometimes it is at the lower ends of the coronal. The second or third place is in the lambdoidal, either by extension from the sagittal, or in a new spot

[2] Bulletins de la Société d'Anthropologie, Paris, 1870.

in one of the branches of the suture. The fourth point is in the coronal near the bregma, and the fifth in the squamous suture of the temporal.

I have studied this question on 100 heads, the ages of which were given, but as they were all from paupers, it is not to be expected that the reports of the ages should in all cases be correct. They are, however, sufficiently so for the very general conclusions I shall draw from them. I regret very much that at first I in many cases did not examine the inside of the skull.

I am not able to say how often this occurred, because at first I often noted the condition of a suture as open, closed, etc., as the case might be, from a study of both sides without, however, stating that fact. It is possible that rather more than one-third of the cases were examined on the outside only. The effect of this will be that as the process begins on the inside, in some cases the records will show less than has occurred. It happily will not lead to any wrong conclusions for, as will appear later, one of the most striking results of these observations is that closure begins much earlier than is generally supposed.

The cases are recorded in order of age. Only the chief sutures of the vault were considered. It was found impossible to tabulate the results satisfactorily, and it seemed best to give a short account of what was found in each case. This method makes it easy to compare the observations with the statements in books.

A word is needed on the terms used. By "open," as applied to a suture, is meant that there is no union between the bones which it separates, so that after maceration they could be taken asunder. By "closed," and "partly closed" is meant that there is more or less bony union between the bones, though the general course of the suture may be perfectly distinct. By "obliterated" is meant that the union is so complete that the suture has disappeared. For this the shorter word "gone" has sometimes been substituted. I fear that in some of my earlier notes I did not always observe this distinction, and may sometimes have put "closed" for "obliterated"; therefore the progress of coösification is, if anything, understated.

Table of One Hundred Observations.

1. Age 17, female, white. Three chief sutures open.
2. Age 21, male, white (Armenian). Very distinct outside and inside; probably open.
3. Age 25, male, white. Ditto.
4. Age 26, female, white. Sutures distinct; lambdoidal shows signs of closing.

5. Age 26, female, white. *Outside:* Obliteration of sagittal and coronal for a short distance at bregma; elsewhere sutures very distinct, with small points of closure in sagittal. *Inside:* Lambdoidal quite open; advanced obliteration of other two.
6. Age 26, male, black. Lambdoidal open; sagittal probably open; coronal obliterated nearly to the top on the *inside;* only at the lower ends on the *outside.*
7. Age 27, male, black. *Outside:* Coronal distinct, sagittal and lambdoidal closing. *Inside:* All three gone.
8. Age 28, male, white. Sutures distinct.
9. Age 28, male, white. Sutures distinct; probably quite separable.
10. Age 29, male, white. *Outside*: Most of coronal and front part of sagittal present, back part of latter and top of lambdoidal obliterated.
11. Age 30, female, white. *Outside:* Coronal distinct except below; sagittal closed; lambdoidal obliterated at apex, visible below. *Inside:* Coronal, obliterated; sagittal closed and in back part obliterated; lambdoidal obliterated below, visible at apex.
12. Age 31, male, white. Sutures partly closed, but distinct on both surfaces; the least distinct is the coronal on the inside.
13. Age 31, male, white. Sutures very distinct; coronal closing in spots.
14. Age 31, male, white. Coronal obliterated, so is sagittal except for about one inch near bregma; lambdoidal closed, but visible.
15. Age 31, female, white. *Outside:* Sutures distinct, but obliteration has begun in back part of sagittal. *Inside:* Chief suture obliterated.
16. Age 32, female, white. Coronal closed at both lower ends and at bregma; sagittal closing in front; lambdoidal open.
17. Age 33, female, white. *Outside:* Sutures distinct; sagittal partly closed. *Inside:* Sagittal partly obliterated.
18. Age 34, male, white. *Outside:* Sagittal obliterated in posterior half, nearly so in front; coronal obliterated below, closed above; lambdoidal distinct. *Inside:* All gone except lambdoidal, of which a trace remains.
19. Age 34, male, white. *Outside:* Sutures closing, but distinct, except lower ends of coronal. *Inside:* All are practically obliterated.
20. Age 35, male, white. *Outside:* Coronal distinct, lambdoidal less so; sagittal nearly obliterated. *Inside:* All nearly gone.
21. Age 55, male, white. Sagittal open; coronal obliterated on *inside*, except at top; on outside, at lower ends, very distinct elsewhere; lambdoidal very clear *outside*, obliterated inside except at top.
22. Age 35, male, white. Coronal distinct; sagittal obliterated except in front; lambdoidal becoming obliterated.
23. Age 35, male, white. Sagittal obliterated except for an inch or so in front; other two pretty distinct, but

closed or closing; lambdoidal especially shows signs of obliteration.

24. Age 35, male, white (Italian). All three open; one point of closure at left lower end of coronal; a frontal suture is also open.
25. Age 36, male, white. Sagittal completely obliterated; other two apparently closing.
26. Age 36, male, white. Coronal probably open throughout, except at lower ends; sagittal obliterated *outside;* distinct *inside*, except at lower ends; lambdoidal on *outside* distinct, beginning to close, contains large Wormian bones. *Inside:* Suture distinct; Wormian bones small or wanting.
27. Age 37, male, white. Sagittal becoming obliterated, especially in front; other two distinct.
28. Age 37, male, white. There was an ulcer near the vertex. All nearly obliterated.
29. Age 37, male, white. *Outside:* Some obliteration of sagittal, others distinct. *Inside:* All gone except part of lambdoidal.
30. Age 37, male, white. All sutures closing; coronal obliterated on lower end on right; sagittal obliterated except in front; lambdoidal distinct.
31. Age 38, female, black. Sagittal open except at hind end; coronal closed at lower end on both sides; lambdoidal open.
32. Age 38, male, white. Sutures closed and beginning to disappear; hind portion of sagittal in part obliterated.
33. Age 38, male, white. Coronal distinct but closed; sagittal obliterated except near bregma; lambdoidal becoming obliterated.
34. Age 38, male, white. A persistent frontal suture is distinct; coronal closed below on both sides, other two distinct.
35. Age 39, female, white. Sagittal obliterated; coronal obliterated at bregma, closing below; lambdoidal becoming obliterated.
36. Age 39, male, white. *Outside:* Sutures very distinct, apparently open, except lower ends of coronals. *Inside:* Lambdoidal distinct, probably open; sagittal distinct behind, obliterated in front; coronal is the most obliterated, quite gone on top.
37. Age 40, female, white. Sutures almost completely obliterated, but visible on inside; very thick skull.
38. Age 40, male, white. Lambdoidal closed; other two distinct.
39. Age 41, male, white. Sutures closed; and, in parts, obliterated.
40. Age 41, male, white. *Outside:* Sagittal partly obliterated; coronal distinct, but closing; lambdoidal apparently open. *Inside:* Sagittal and coronal visible, but closed.
41. Age 41, male, black. Closure of sutures well advanced.
42. Age 41, female, white. *Outside:* Coronal distinct, except at lower ends; sagittal distinct; lambdoidal visible in places. *Inside:* Coronal obliterated; other two nearly so.

43. Age 41, female, white. *Outside:* Sagittal obliterated; coronal also, except on top; traces of lambdoidal. *Inside:* All gone, except traces of lambdoidal at apex.
44. Age 42, male, white. *Outside:* Sutures closed, but in most places distinct; obliterated in spots, especially lower ends of coronals. *Inside:* Sutures in general obliterated, occasional traces left.
45. Age 42, male, white. Coronal distinct; sagittal closed in most parts, obliterated behind; lambdoidal closing.
46. Age 43, male, white. Sutures distinct, except lambdoidal, which is in places obliterated.
47. Age 43, male, white. Coronal distinct; other two almost completely obliterated.
48. Age 43, male, white. *Outside:* Sutures nearly obliterated. *Inside:* Perhaps a trifle less obliterated than outside, especially apex of lambdoidal.
49. Age 44, female, white. *Outside:* Sutures distinct, apparently open. *Inside:* Almost wholly obliterated.
50. Age 45, male, white. *Outside:* Coronal very distinct; sagittal distinct, but closed; lambdoidal closing. *Inside:* Coronal almost obliterated; sagittal obliterated in places about the same as on outside; lambdoidal closing in the lower parts.
51. Age 45, male, black. Coronal nearly obliterated; sagittal so in parts; lambdoidal distinct, but closed.
52. Age 45, female, white. Obliteration beginning in the three chief sutures; lambdoidal contains Wormian bones, probably for the most part free.
53. Age 46, male, white. *Outside:* Sutures distinct, but closing, especially sagittal. *Inside:* All nearly obliterated, except apex of lambdoidal.
54. Age 47, male, white. *Outside:* Sagittal obliterated; others visible, but obliteration beginning. *Inside:* All closed.
55. Age 47, male, white. Distinct, but beginning to be obliterated.
56. Age 49, male, dark (cross between Indian and Mexican). Obliteration beginning in upper half of coronal on each side; sagittal obliterated, except in front; coronal gone.
57. Age 50, female, white. *Outside:* Coronals distinct; others nearly obliterated. *Inside:* All obliterated.
58. Age 50, female, white. *Outside*: All pretty distinct; hind part of sagittal apparently closing. *Inside:* All closed, or closing.
59. Age 51, male, white. Sutures distinct; but obliteration beginning, especially in posterior half of sagittal; it is more advanced in right side of lambdoidal than in left.
60. Age 51, female, white. Coronal visible on left, obliterated on right; sagittal obliterated; lambdoidal closed, but not obliterated.
61. Age 51, male, white. *Outside:* Sutures tolerably distinct, except lambdoidal. *Inside:* All obliterated.
62. Age 51, female, white. Sutures distinct, but closing.

63. Age 54, male, white. Back part of sagittal and top of lambdoidal beginning to be obliterated; the rest distinct, including a frontal suture.
64. Age 55, male, white. Coronal closed at both lower ends; sagittal closing; lambdoidal closing, complicated with Wormian bones.
65. Age 55, male, white. *Outside:* Lambdoidal obliterated on right, nearly so on left; sagittal nearly obliterated in back part; coronal distinct. *Inside:* All obliterated.
66. Age 56, female, white. *Outside:* Coronal pretty distinct, though closed; others obliterated. *Inside:* All obliterated.
67. Age 60, female, white. Sutures closing in many places; front of sagittal obliterated; top of coronal and lower part of lambdoidal nearly so.
68. Age 60(?), female, white. *Outside:* Sutures visible, but becoming obliterated. *Inside:* Obliterated.
69. Age 62, male, white. Coronal distinct, but closed; others nearly obliterated.
70. Age 63, male, white. Sutures nearly obliterated; right side of coronal least so; some remains of frontal.
71. Age 64, female, white. Chief sutures distinct; little, if any, coössification.
72. Age 64, female, white. *Outside*: Sutures apparently open. *Inside:* All gone, except apex of lambdoidal.
73. Age 65, male, white. *Outside:* Coronal distinct, except at lower ends; sagittal obliterated; lambdoidal nearly gone. *Inside:* Coronal and sagittal obliterated; lambdoidal nearly so.
74. Age 65, male, white. *Outside:* Traces of coronal, the rest gone. *Inside:* All gone, except a trace of apex of lambdoidal.
75. Age 65, male, white. Coronal distinct; lambdoidal obliterated; sagittal visible in front, and gone behind.
76. Age 65, male, white. *Outside:* As in preceding case. *Inside:* Precisely the converse, except that lambdoidal is not clear.
77. Age 65, male, white. Sagittal nearly obliterated; other two closing.
78. Age 66, male, white. Obliteration in various places. Lower ends of coronal and front of sagittal gone.
79. Age 67, female, white. Coronal partly closed; sagittal distinct, slight union behind; lambdoidal apparently open.
80. Age 68, male, white. *Outside:* Closed but visible, including a frontal suture. *Inside:* Barely visible.
81. Age 68, male, white. Coronal distinct, but closing; other two obliterated.
82. Age 68, female, white. *Outside:* Distinct, though obliteration is beginning in sagittal and lambdoidal. *Inside:* Obliteration all but complete.
83. Age 70, female, white. Sutures distinct; apparently not closed.
84. Age 70, male, white. *Outside:* Visible, but obliterating, especially the sagittal. *Inside:* All practically gone; a trace of apex of lambdoidal.

85. Age 70, male, white. *Outside:* Slight traces of sutures, especially of coronal. *Inside:* All practically obliterated.
86. Age 71, male, white. *Outside:* All very distinct, except lower ends of coronals, which are gone. *Inside:* All obliterated, except apex and left side of lambdoidal.
87. Age 71, male, white. *Outside:* Sutures irregularly closed. *Inside:* Obliterated.
88. Age 72, male, white. Sutures perfectly distinct; little, if any, obliteration.
89. Age 75, male, white. Lambdoidal and back of sagittal nearly obliterated; the rest distinct.
90. Age 75, male, white. Lambdoidal gone; sagittal also, except for an inch or so in front; coronal distinct on right, nearly obliterated on left, especially below.
91. Age 75, male, white. *Outside:* Traces of sutures, including upper part of frontal. *Inside:* All gone.
92. Age 75, male, white. *Outside:* Coronal visible, but obliterating; sagittal visible in front, gone behind; lambdoidal visible in places. *Inside:* all obliterated, except apex of lambdoidal.
93. Age 75, male, white. *Outside:* Coronal distinct, except at lower ends; all else gone. *Inside:* All gone.
94. Age 79, male, white. Chief sutures nearly obliterated; lambdoidal perhaps the least.
95. Age 80, female, white. Chief sutures perfectly distinct; no sign of obliteration, though there may be some union; thin skull.
96. Age 80, female, white. Sutures closed, but lines distinct.
97. Age 81, female, white. *Outside:* Coronal distinct, but closed; sagittal obliterated; lambdoidal visible in spots. *Inside:* All gone, except apex of lambdoidal.
98. Age 82, female, white. Sutures can be made out, though in parts obliterated; a frontal suture is by far the most distinct.
99. Age 82 (?), male, white. A frontal suture is distinct in most places, though obliterated at lower end and in other parts; coronal obliterated at lower ends, elsewhere distinct; sagittal obliterated behind, distinct in front; lambdoidal contains Wormian bones; it is pretty distinct.
100. Age 91, female, white. *Outside:* Coronal very distinct; sagittal closed in hind portion, but visible, very distinct elsewhere; lambdoidal almost obliterated. *Inside:* Sagittal completely obliterated; the other two nearly so.

The most evident conclusions from this table seem to be:

(1) That the sutures begin to close much earlier than has ever been stated. The process is apparent in several cases of persons under thirty. Of those from thirty to forty there is but a single case (No. 24, an Italian of thirty-five) in which closure has not made

progress, and in this one it is beginning. In another (No. 36) there is but little union to be seen on the outside.

(2) That the closing almost invariably begins on the inside, which was well-known before. It seems, however, that the process does not at all necessarily appear first on the outside opposite the points previously attacked on the inside. On the contrary, in several cases one part of a suture is obliterated on the outside, and the other on the inside. In (No. 76) the inside of the skull of a women of sixty-five, shows almost precisely the converse of the ossification of the outside. What is open on one surface is closed on the other except for the lambdoidal.

(3) That the time of closure of any particular part of a suture, and the order in which the process advances are very uncertain. In proof of this we need only compare the appearances of the individual cases of any group of five or six skulls of the same or nearly the same age in the first half of the table. As to the usual order I shall venture only to say what I *think* about some points.

I think that closure generally begins in the back part of the sagittal and often as soon or nearly as soon in the lower ends of the coronal. I think that when the sutures close early the coronal usually closes before the lambdoidal, but that in old skulls, on the outside, at least, the lambdoidal is more frequently obliterated than the coronal. On the inside of old skulls there is very often a minute line showing the position of the apex of the lambdoidal suture when all the others are quite gone. A persistent frontal suture is one of the last to disappear as has been previously taught.

As to the rules for determining the age of the skull from the condition of the sutures, it is necessary only to compare them with the observations recorded in this table to see what they are worth. It must not be forgotten that there are other guides to the age of the skull; and I am not prepared to assert that, taken together with them, the sutures are absolutely worthless in the hands of an experienced anatomist. I am sure that to any one else the rules in question are misleading and dangerous.

MÉTHODE DE DÉMONSTRATION

DES

MOUVEMENTS LARYNGIENS

ÉTUDE DU LABORATOIRE

DE PHYSIOLOGIE DE L'ÉCOLE DE MÉDECINE DE HARVARD

PAR

Le D[r] FRANKLIN H. HOOPER

DE BOSTON

MÉMOIRE LU AU CONGRÈS INTERNATIONAL DE MÉDECINE DE BERLIN

AOUT 1890

(Extrait des *Annales des maladies de l'oreille et du larynx.*)

PARIS

G. MASSON, ÉDITEUR

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1890

MÉTHODE DE DÉMONSTRATION

DES

MOUVEMENTS LARYNGIENS

Les méthodes décrites dans ce travail sont celles qui ont été employées par le professeur Henry P. Bowditch et par moi durant ces dernières années pour étudier les mouvements des cartilages du larynx et l'action des muscles laryngiens intrinsèques. Pour rendre cette communication aussi complète que possible, il est nécessaire de mentionner brièvement les travaux publiés antérieurement.

Les mouvements laryngiens sont effectués, soit par l'air expulsé des bronches, soit par l'action des muscles insérés aux cartilages du larynx. L'action de certains muscles intrinsèques du larynx sera mieux appréciée et fixée dans la mémoire en considérant d'abord quelques-unes des propriétés particulières des cartilages et leurs mouvements produits par le souffle de l'air. Ces cartilages peuvent se mouvoir librement l'un sur l'autre, et leurs surfaces articulaires sont particulières et uniques. Le thyroïde est le plus grand des cartilages du larynx, et la première chose qui frappe celui qui le dissèque est la force des muscles extrinsèques qui y sont insérés. Ces muscles servent à l'assujettir.

Le cartilage inférieur par la taille, le cricoïde, contraste d'une manière frappante avec le thyroïde sous le rapport des muscles, puisque, à l'exception de quelques fibres du constricteur inférieur du pharynx qui passent par dessus son bord postérieur, aucun muscle extrinsèque n'y est inséré. Il n'y a, par conséquent, rien qui puisse empêcher le jeu libre du cartilage cricoïde sur le thyroïde.

En observant extérieurement ces cartilages, on supposerait naturellement que le thyroïde, le plus grand et le mieux

assujetti des cartilages, est le point fixe sur lequel le cricoïde se meut librement, et on trouvera par l'expérimentation que c'est vraiment le cas. Si nous excitons les nerf laryngés soit supérieurs, soit inférieurs, et si nous surveillons les effets sur les cartilages, nous n'observons aucun mouvement appréciable du thyroïde, tandis que, dans certaines conditions, on observe un mouvement ascensionnel très prononcé du cricoïde sur le thyroïde. Mais nous ne pûmes obtenir, dans un aucun cas, un mouvement descendant du cartilage thyroïde sur le cricoïde par l'irritation de ces nerfs ; et il est douteux qu'un tel mouvement s'observe ja-

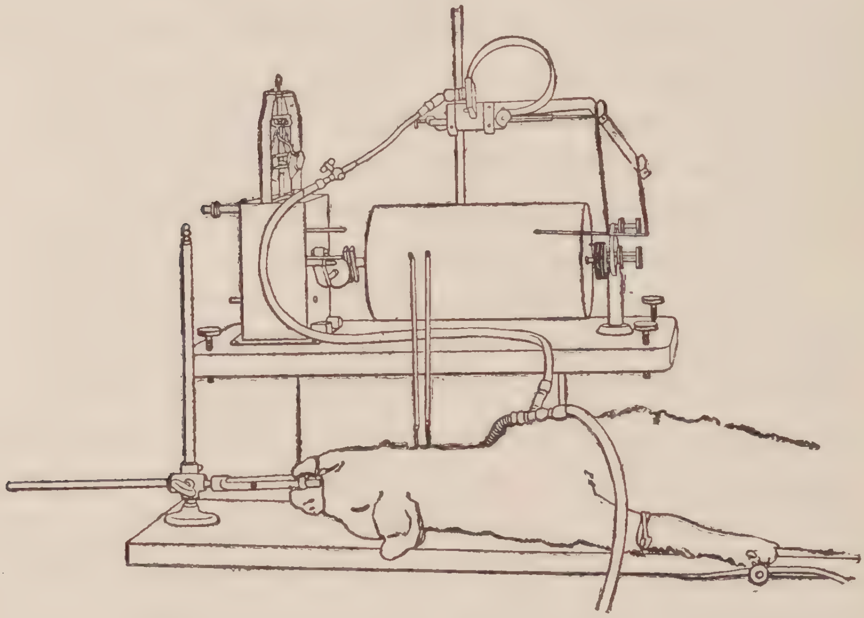

Fig. 1.

mais parmi les fonctions remplies par le larynx. Dans l'expiration forcée et dans la vocalisation, il existe un mouvement ascensionnel de tous les cartilages du larynx ; mais même il y a toujours, en ce cas, un excès de mouvement du cartilage cricoïde sur le thyroïde.

Pour démontrer expérimentalement les mouvements des cartilages sous l'influence d'une tension d'air croissant dans la trachée, comme cela se produit en faisant résonner des notes élévées, un chien de bonne taille, récemment abattu, doit être arrangé comme dans la figure 1.

On verra que le sternum du chien a été enlevé et une ca-

nule en forme de T assujettie à l'extrémité supérieure de la trachée divisée. Un bras de la canule communique avec un manomètre de Fick, qui enregistre la pression de l'air insufflé dans la trachée, tandis que l'autre bras est tenu par l'expérimentateur. La portion du larynx située au-dessus des cordes vocales a été oblitérée par de l'ouate sur laquelle on a répandu du plâtre de Paris pour durcir l'ouate et fermer le larynx suffisamment pour prévenir la fuite de l'air. Deux leviers, de 28 centimètres de long, faits avec de la paille ordi-

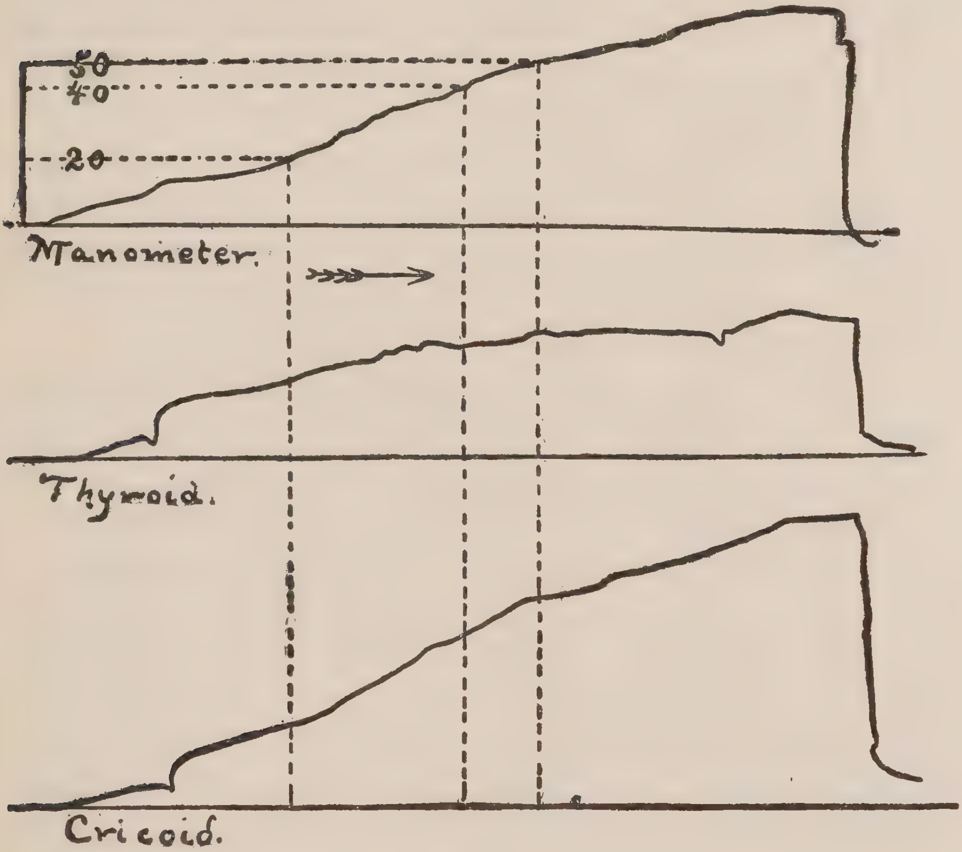

Fig. 2.

naire et terminés à une extrémité par une forte épingle, sont fixés au centre des cartilages thyroïde et cricoïde. L'extrémité la plus éloignée de chaque levier étant garnie d'une délicate pointe métallique, pour enregistrer sur un papier fumé, posé sur un cylindre tournant, les courbes faites par les cartilages se mouvant sous la pression de la colonne d'air venant de la trachée. Les tracés de la figure 2 doivent être lus de gauche à droite.

Les lignes verticales portant les numéros 20, 40, 50 représentent la pression en millimètres de mercure, le manomètre de Fick ayant été gradué expérimentalement. Les lignes pointillées montrent la position des leviers ascendants, attachés aux cartilages, à des pressions correspondantes.

On verra que, à pression égale, la courbe du cricoïde au-dessus de la ligne de zéro dépasse celle du thyroïde. Par la mensuration, nous avons à une pression de 20 millimètres de Hg une ascension de 8 millimètres du levier cricoïde, tandis que celle du thyroïde à la même pression est de 7. Si la force du souffle est augmentée, la différence est plus marquée. A une pression de 40 millimètres Hg, le cricoïde atteint 16 millimètres, le thyroïde $10^{mm},5$. A une pression de 50 millimètres Hg, l'accroissement est encore plus grand. Sous cette pression le cricoïde arrive à $19^{mm},5$ contre 12 millimètres du thyroïde. A chaque pression toutefois, nous avons un excédent de mouvement des cartilages cricoïdes sur les thyroïdes.

Ces expériences ont été répétées sur neuf chiens différents, avec des résultats confirmatifs qui rendent évident que la tension accrue de l'air dans les bronches produit un mouvement ascensionnel prononcé du cartilage cricoïde, en plus de l'ascension générale du larynx, mouvements qui croissent en proportion de la force avec laquelle l'air est expiré de la poitrine. D'abord nous fûmes embarrassé d'expliquer la cause de ce phénomène, mais on finit par décider que l'expansion du larynx lui-même, *quand il est gonflé*, devait en donner l'explication.

On détermina par l'expérimentation que la capacité du larynx était augmentée et non diminuée par un mouvement ascensionnel du cartilage cricoïde ; de la sorte, dans les violents efforts phonétiques, dus à l'expansion du larynx, le cricoïde doit se mouvoir sur le thyroïde, puisque, en accomplissant cette action, il accroît la capacité du larynx (1). Par ce mouvement ascensionnel de la portion antérieure du car-

(1) Pour plus ample description des expériences qui nous ont amené à ces conclusions, nous renvoyons le lecteur à l'article original : Experimental researches in the tension of the vocal bands (Translated from *the American Laryngological Association*, 1883).

tilage cricoïde, sa plaque postérieure, avec tout ce qui y est inséré, est nécessairement portée en haut et en arrière, tendant de la sorte les cordes vocales. Notre opinion est que la tension longitudinale des cordes vocales est toujours produite de cette manière, et jamais par un mouvement du cartilage thyroïde sur le cricoïde. Nous considérons aussi l'expiration du souffle comme un des moyens les plus importants par lesquels on aboutit à la tension des cordes vocales. Un autre tenseur important est le muscle thyro-cricoïdien. Son action est la même que celle du souffle qui vient d'être décrite, et peut être démontrée en disposant un chien comme dans la figure 3.

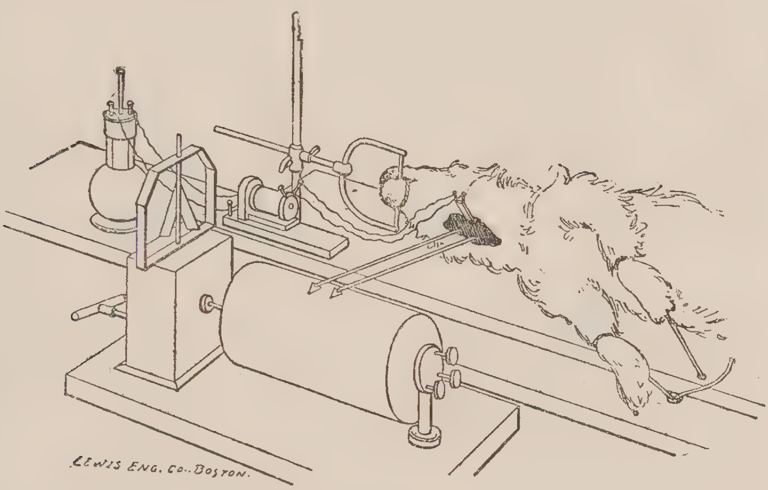

Fig. 3.

On verra que le chien est couché sur le côté et est maintenu par une poignée appropriée sur une table. Les deux nerfs supérieurs laryngés ont été mis à nu et placés sur des électrodes couverts. Les mêmes leviers qui avaient servi aux expériences précédentes sont fixés au centre des cartilages thyroïde et cricoïde préalablement mis à nu par la dissection. Ces leviers enregistrent graphiquement sur un papier fumé posé sur un cylindre tournant les mouvements faits par les cartilages quand les nerfs laryngés supérieurs sont irrités au moyen d'un appareil d'induction ordinaire.

Dans les tracés de la figure 4, l'astérisque désigne le commencement de l'excitation, et la flèche la fin.

Au point d'excitation, on remarquera que le cartilage cricoïde était tiré en haut vers le thyroïde, ainsi que c'est indiqué par l'élévation marquée de la courbe cricoïde. Sitôt que l'irritation cessait, le levier tombait immédiatement à son niveau précédent. Toutefois, le cartilage thyroïde ne ressentait aucun effet appréciable. La légère ondulation de la ligne thyroïde doit être mise sur le compte du mouvement général du larynx causé par la contraction du muscle thyroïdien, mais

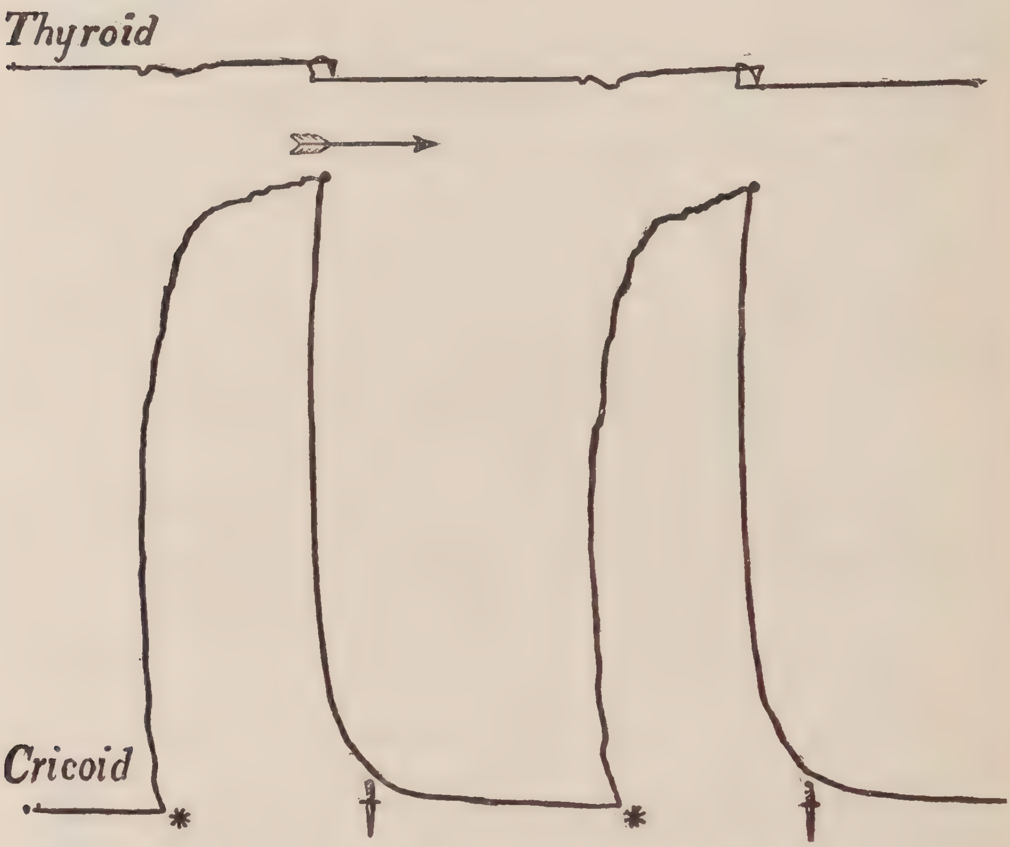

Fig. 4.

il est évident qu'aucun mouvement positif n'est imprimé au cartillage typhoïde par son action.

Ces expériences ont été répétées sur différents chiens avec des résultats confirmatifs. Ils nous obligent à nous mettre d'accord avec Magendie (1), Longet (2), Schech (3) et

(1) Mémoire sur l'usage de l'épiglotte dans la déglutition. Paris, 1813.
(2) Longet : Recherches expérimentales sur les nerfs des muscles du larynx, etc. (*Gazette médicale de Paris*, 1841.)
(3) Schech : *Experimentelle Untersuchungen über die Funktionen der Nerven und Muskeln des Kehlkopfs*, IX, 1873.

Schmidt (1) qui sont arrivés à de semblables résultats par l'observation expérimentale et de rejeter la théorie généralement acceptée, que le cartilage thyroïde est tiré en bas par la contraction du muscle. La tension longitudinale des cordes vocales est le résultat direct de ce mouvement du cartilage cricoïde sur le thyroïde, et le souffle expiratoire et les muscles thyro-cricoïdiens sont regardés par nous comme les plus importants, sinon les seuls tenseurs de ces ligaments élastiques.

En passant à présent à l'articulation crico-aryténoïdienne, nous sommes en présence d'une articulation très particulière, n'ayant rien de semblable de toute autre partie du corps. Nous trouvons alors que le cartilage cricoïde est le point fixe, tandis que les cartilages aryténoïdiens sont actifs et se meuvent librement. A ces derniers sont insérés des systèmes de muscles qui portent sur deux fonctions distinctes et séparées, — ceux qui tiennent la glotte ouverte pour la respiration, et ceux qui la ferment pour la phonation.

L'articulation crico-aryténoïdienne admet toutes les variétés de mouvement. La facette articulaire du cartilage cricoïde est convexe, celle de l'aryténoïde est concave. La facette du cartilage cricoïde, elliptique dans son contour, a une surface à peu près cylindrique, l'axe du cylindre étant dirigé en avant, en dehors et en bas. Le plus long diamètre de la facette convexe allongée du cartilage cricoïde étant presque à angle droit avec le plus grand diamètre de la facette concave du cartilage aryténoïdien, un mouvement extensif à coulisse du dernier cartilage sur le cricoïde est rendu possible. Les mouvements du cartilage aryténoïdien sur le cricoïde paraissent être soit une rotation autour de l'axe du cylindre décrit ci-dessus, soit un mouvement en coulisse, dans la direction de l'axe, ou une combinaison des deux.

Ces mouvements peuvent être démontrés en attachant le larynx excisé à une poignée (fig. 5) arrangée de façon à ce

(1) Schmidt : *Die Laryngoscopie auf Thieren*, 1873.

que les muscles puissent être excités soit individuellement, soit collectivement.

Ce mode de préhension du larynx, comme on le voit, consiste en une charpente de bois à laquelle sont attachées trois rangées de bornes. A ces bornes sont attachés des fils métalliques terminés par des hameçons dont quatre sont courbés et deux sont droits. Le larynx excisé d'un gros chien auquel on a retiré les ailes du cartilage thyroïde et mis à nu les muscles,

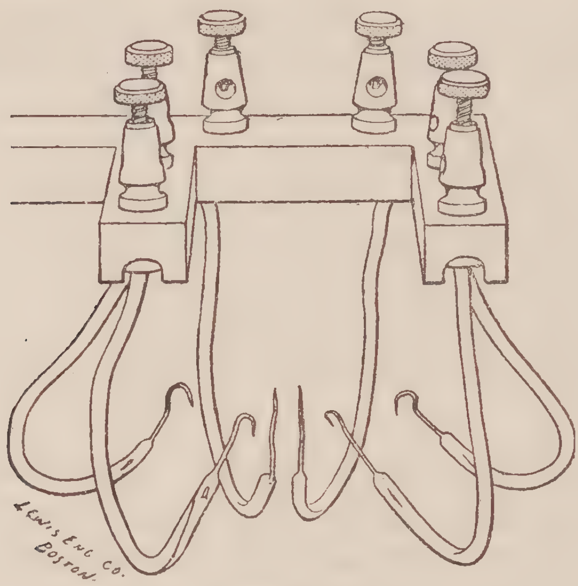

Fig. 5.

est chevillé au centre de l'appareil. Les hameçons courbes sont alors insérés dans le crico-aryténoïdien postérieur et les muscles crico-aryténoïdiens latéraux, tandis que les hameçons droits sont introduits dans la substance des cordes vocales, les muscles thyro-aryténoïdiens internes. Trois appareils d'induction séparés sont en communication avec les bobines; de sorte que les différents systèmes de muscles peuvent, comme il a été dit précédemment, être excités séparément ou simultanément. L'arrangement du larynx pour ces démonstrations est démontré par la figure 6.

Quand ces trois systèmes de muscles étaient excités par un courant de même intensité, on obtenait une occlusion complète de la glotte, montrant que l'action combinée des muscles crico-aryténoïdiens internes et latéraux surpasse

en puissance les dilatateurs de la glotte, les crico-aryténoïdiens postérieurs.

En irritant les muscles crico-aryténoïdiens postérieurs, les cartilages aryténoïdiens étaient tournés en dehors; il en résultait une dilatation complète de la glotte. Si l'on stimulait les muscles crico-aryténoïdiens internes ou latéraux, soit individuellement, soit ensemble, il survenait une occlusion de la glotte.

L'excitation simultanée des muscles latéraux et des postérieurs produisait la dilatation de la glotte, montrant que les postérieurs dépassaient comme puissance les latéraux.

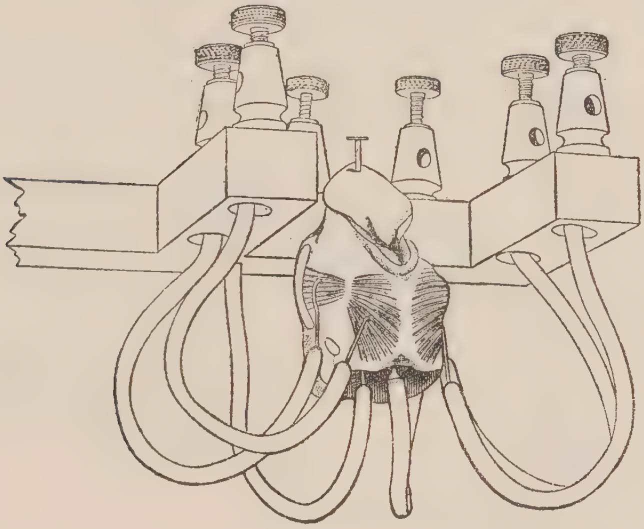

Fig. 6.

Quand on excitait de la même manière les muscles internes et les postérieurs, nous obtenions une occlusion de la glotte en avant et une ouverture en arrière, donnant l'apparence familière connue sous le nom de mouvement mixte.

Les démonstrations ci-dessus rendent très claire l'action de ces muscles, mais elle a son désavantage en ce sens que les muscles perdent leur irritabilité en 10 à 25 minutes et ne répondent pas plus longtemps à la stimulation.

Une autre méthode pour rechercher l'action des mouvements laryngiens intrinsèques est d'arranger un chien d'une bonne taille comme dans la figure 7.

Ici, le larynx a été retiré du cou sans blesser les grands vaisseaux sanguins ou les nerfs. Les côtés des cartilages

thyroïdes ont été retirés pour mettre à nu les muscles crico-aryténoïdiens latéraux et internes. La membrane muqueuse a été aussi séparée du muscle transverse et des crico-aryténoïdiens postérieurs. Chacun de ces muscles peut donc être stimulé directement, et nous considérons cette méthode comme une des meilleures par lesquelles on puisse étudier l'action des muscles laryngiens intrinsèques.

Si l'on désire observer les cordes vocales en bas, la canule représentée dans la figure 8 sera commode.

Pour l'insérer, il faut diviser complètement la trachée à une petite distance au-dessous du larynx. L'extrémité su-

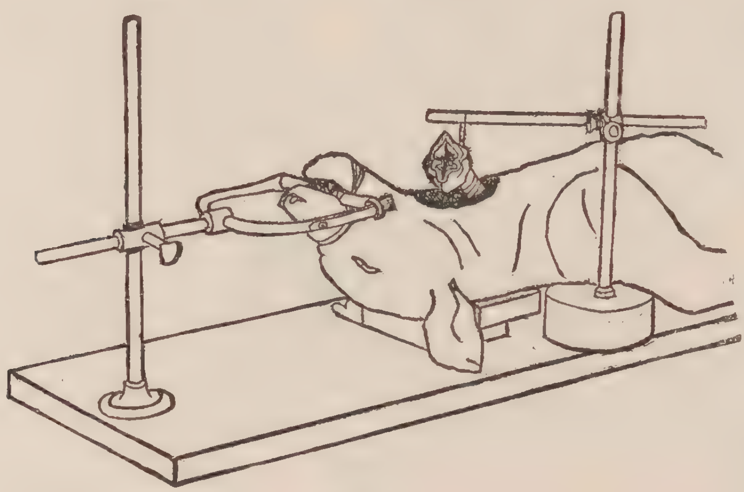

Fig. 7.

périeure de la trachée est alors attachée à la canule à la lettre A, et l'extrémité inférieure à lettre B. A C se trouve adaptée à la canule une pièce de laiton mobile dont la tête est fermée par une plaque de verre cimentée obliquement au-dessus. L'obliquité du couvercle empêche que la lumière ne soit réflétée dans l'œil de l'observateur, et une goutte de glycérine placée sur la surface inférieure empêche le verre de devenir opaque par la condensation de l'humidité de l'haleine. Quand le couvercle est en place, la respiration se fait par la voie naturelle à travers les fosses nasales et le larynx. En enlevant le couvercle, les cordes vocales peuvent être stimulées directement sur leur surface inférieure par la canule à la lettre C.

Il ne faut pas présumer qu'il n'existe pas d'autres mouvements laryngiens à côté de ceux qui sont démontrables par les méthodes dont je viens de parler dans cette communication. L'action des muscles intrinsèques du larynx est si compliquée et si délicate qu'il ne semble pas possible, avec les appareils que nous possédons à présent, de les enregistrer tous graphiquement. En les étudiant, toutefois, par les méthodes décrites plus haut, et en les classant d'accord avec les fonctions du larynx, on aura une conception claire de leurs principaux mouvements. La fonction principale du larynx est celle de la respiration et une seule paire de muscles y préside, les crico-aryténoïdiens postérieurs. La stimulation de ces muscles, par l'une des méthodes démontrées dans les figures 5 et 7, est suivie de la rotation des cartilages aryté-

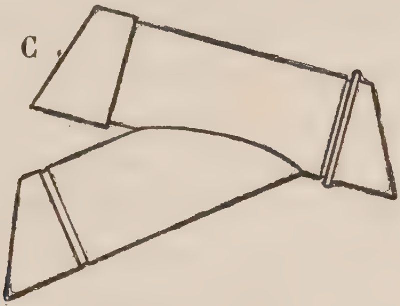

Fig. 8.

noïdiens en dehors, et par conséquent de la dilatation de la glotte. Les muscles respiratoires tiennent la glotte ouverte pour permettre le passage de l'air, et sont probablement les seuls dilatateurs du larynx.

Les muscles qui exécutent la fonction phonatrice du larynx sont les tenseurs longitudinaux et les obturateurs.

Les tenseurs, les muscles thyro-cricoïdiens, tendent les cordes vocales, comme on le voit dans la figure 4, en tirant le cartilage cricoïde sur le thyroïde.

Les obturateurs de la glotte sont les thyro-aryténoïdiens internes, les crico-aryténoïdiens latéraux et le transverse.

L'occlusion des thyro-aryténoïdiens internes peut être observée en appliquant les électrodes sur les surfaces supérieures des cordes vocales. Si on les excite de cette manière avec un très faible courant, les cordes se fermeront hermé-

tiquement en même temps. Si on emploie un fort courant, il se produit en plus de l'occlusion une contraction des cordes dans la direction antéro-postérieure, les cartilages aryténoïdiens étant tirés légèrement en avant. Cette paire de muscles est la plus compliquée du groupe laryngé, et probablement elle agit dans plusieurs autres sens, à côté de ceux que je viens d'énumérer, en réglant la forme de la fente de la glotte et l'épaisseur des cordes vocales pendant la production des diverses variétés de sons vocaux.

L'action des crico-aryténoïdiens latéraux peut être démontrée par l'une ou l'autre des méthodes exposées dans les figures 5 et 7. Ce sont de puissants obturateurs, et ils agissent en imprimant aux cartilages aryténoïdiens un mouvement de rotation en dedans.

Les muscles transverses agissent en commun avec les autres obturateurs et se rapprochent des cartilages aryténoïdiens par un mouvement en coulisse en dedans.

Les mouvements de la glotte peuvent aussi être étudiés par la stimulation directe des nerfs récurrents laryngés. Mais cette branche du sujet reste en dehors du but du présent mémoire, et est trop compliquée pour qu'on se permette d'en faire plus qu'une brève mention.

Nous dirons simplement que, dans notre expérience, l'effet produit sur la glotte, lorsque les nerfs récurrents sont irrités, dépend du degré de stimulation et des anesthésiques employés. La dilatation de la glotte chez les chiens peut être produite par des stimulations lentes (18 à 20 chocs d'induction à la seconde) et par l'effet de l'éther sulfurique. Si un chien est profondément sous l'influence de l'éther, la dilatation de la glotte suivra la stimulation des récurrents, sans égard au degré ou à l'intensité de la stimulation ; mais si l'animal est légèrement éthérisé, ou si l'on emploie un autre anesthésique, la dilation ne se produira que si le nombre des stimulations est au-dessous de 30 à la seconde ; les courants les plus rapides produisent l'occlusion de la glotte (1).

(1) Pour les détails de ces observations, nous renvoyons les intéressés à nos travaux initiaux sur ces sujets :

Chez le chat, toutefois, le résultat habituel d'une semaine de forts courants appliqués aux nerfs récurrents est une dilatation de la glotte; mais un nombre très rapide de vibrations (80 à la seconde) produira aussi une occlusion chez cet animal.

Les mouvements de la glotte évoqués par l'irritation du centre cortical cérébral ont été étudiés avec beaucoup de soin et de talent par les professeurs H. Munk et H. Krause, de Berlin, et le Dr F. Semon et le professeur Horsley, de Londres; mais jusqu'à présent nous ne les avons pas recherchés.

a. The respiratory function of the human larynx (*New-York med. Journal*, July 4th, 1885).
b. The anatomy and physiology of the recurrent laryngeal nerves. (*New-York med. Journal,* July 9, 16, 23th and August 6th, 1887.)
c. Effects of varying rates of stimulation on the action of the recurrent laryngeal nerves. (*New-York med. Journal.* November 3rd, 1888.)

Paris. — Soc. d'Imp. PAUL DUPONT, 4, rue du Bouloi (Cl.) 1692.12.90.

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THE KNEE-JERK AND ITS PHYSIOLOGICAL MODIFICATIONS.

BY H. P. BOWDITCH, M.D., *Professor of Physiology, Harvard Medical School,* AND J. W. WARREN, M.D., *Instructor in Physiology, Harvard Medical School.*

THE study of the modifications of the knee-jerk brought about by the simultaneous activity of other portions of the nervous system is important both on account of the evidence as to the real nature of the knee-jerk which may thus be obtained and because an investigation of these phenomena is likely to throw light upon the mysterious processes of the central nervous system.

That the exaggerated tendon reflexes observed in certain diseases of the central nervous system can be inhibited by the stimulation of the skin or of the peripheral nerves has been noticed by various observers[1], but the reflexes thus inhibited were in nearly all the cases reported the clonic movements produced under morbid conditions by stretching the tendons of the muscles concerned. Erb, however, mentions a single case of myelitis from compression in which the tendon reflex as well as the trembling of the legs were stopped at once by pinching the skin of the abdomen.

There is good reason to believe that the mechanism of these clonic movements is the same as that of the ordinary knee-jerk, and it might therefore naturally be expected that both phenomena would be similarly influenced by peripheric nerve stimulation, but that the normal knee-jerk of a healthy individual can be thus inhibited has not yet, as far as we are aware, been a matter of recorded observation.

On the other hand the reinforcement of the normal knee-jerk by various forms of activity of the nervous system has during the last few years been made a subject of careful study by many independent observers. Jendrássik[2] first called attention to the fact "that if, when the patellar tendon is struck, the patient clinches the hand, or

[1] Cf. Nothnagel, *Arch. f. Psych.* VI. S. 332, 1876. Lewinski, *Arch. f. Psych.* VII. S. 327, 1877. Erb, *Ziemssen's Handbuch d. spec. Path. u. Ther.* II. Aufl. XI. II. S. 59.

[2] Jendrássik: "Beiträge zur Lehre von den Sehnenreflexen." *Deutsches Arch. f. klin. Med.* 1883, XXXIII. 177.

makes other violent movement, the coincident jerk is increased." He also thought that stimulation of the sensory nerves had a similar influence upon the tendon reflex, but considered his experiments on this point incomplete and that such an influence was more difficult of determination.

Mitchell and Lewis[1], in a careful study of the circumstances which increase and those which lessen the knee-jerk, observed a reinforcement consequent not only upon volitional acts directed to other parts of the body, but upon painful stimulation of the nerves of the skin either by pinching or by the application of heat, cold or electricity. A similar reinforcement was noticed when the eyes were exposed to the light of burning magnesium wire. A considerable normal variation of the knee-jerk was noted by these observers and some of the causes thereof were determined.

The variations of the normal knee-jerk were further studied by Lombard[2], who demonstrated that "fatigue, hunger, enervating weather and sleep, conditions which decrease the activity of the whole central nervous system, decrease the average knee-jerk, while rest, nourishment, invigorating weather and wakefulness, influences which increase the activity of the central nervous system, increase the average knee-jerk." Thus it was found that there was a diurnal variation in the knee-jerk, the maximum occurring as a rule in the morning soon after breakfast. Sensory irritations, voluntary movements and strong emotions when synchronous with the blow, were found to increase the knee-jerk.

The statement by Mitchell and Lewis[3], "that the muscular action or circuit closing, must precede the tap, in order to reinforce it, by a period which is, as yet, undetermined," suggested the importance of studying the exact relations in time between the knee-jerk and the reinforcing act, and the experiments here recorded were undertaken with the object of throwing light upon this subject.

A brief preliminary report of some of the earlier results which were obtained was presented at a meeting of the National Academy of Sciences at Washington, April 17th, 1888[4], and the present paper has for its object to place upon record a detailed account of the method of research together with such conclusions as seem to be justified at the present stage of the investigation.

[1] *Medical News*, Feb. 13 and 20, 1886.

[2] *American Journal of Psychology*, I. 1. 1887.

[3] *loc. cit.* Reprint, p. 24.

[4] *Boston Med. and Surg. Journal*, May 31, 1888.

In these experiments it was necessary:

(1) To deliver upon the same part of the *ligamentum patellae* a series of blows of constant intensity and direction.

(2) To record the extent of the knee-jerks thus produced.

(3) To study the effect upon the knee-jerks of calling the central nervous system into momentary activity either by voluntary motor impulses or by sensory impressions standing in definite time relations to the blow upon the tendon.

The pieces of apparatus employed in the research were therefore the following:

(1) An electrical hammer.

(2) A recording apparatus.

(3) A pendulum interrupter.

Description of Apparatus.

In order to secure a certainty that the blow would always fall upon the same point during each experiment Rosenheim's[1] plan of attaching the hammer to the lower leg was, after some preliminary experimenting with other methods, finally adopted. This portion of the apparatus is shown in Fig. 1. The light wooden hammer *H*, and the electro-

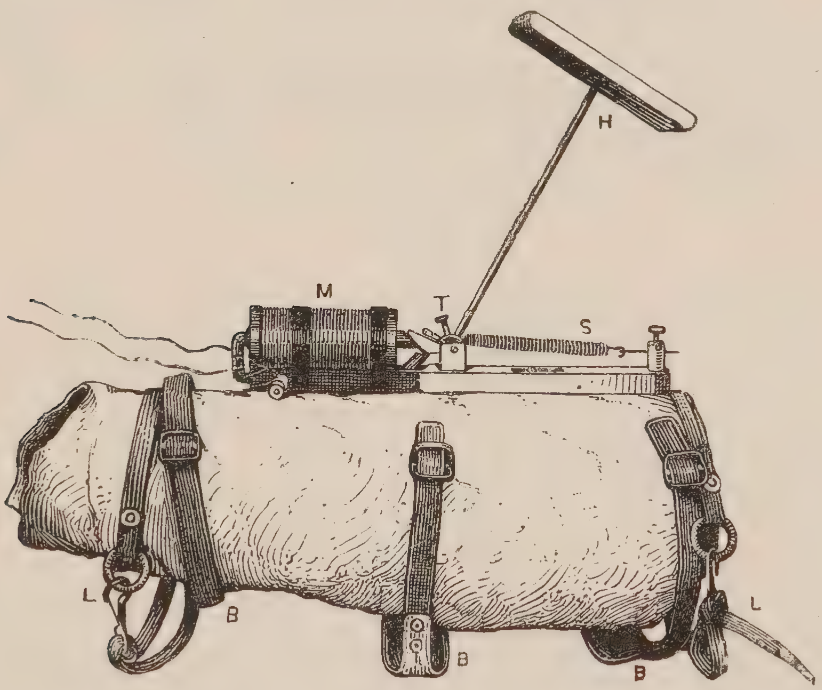

Fig. 1.

magnet *M*, holding it uplifted, were fixed upon a splint-like covering made of plaster of Paris and coarse gauze enclosing the lower leg. The

[1] *Archiv für Psych.*, xv. 192.

force of the blow was regulated by the tension of a spiral spring *S*, which drew the hammer toward the knee when it was released by the breaking of an electric current controlling the magnet. The straps, *B, B, B,* served to secure the apparatus to the leg, and the rings *L, L* to suspend the leg from the ceiling as shown in Fig. 2.

The subject of the experiment lay upon his right side with the left knee slightly bent (Fig. 2) and the internal condyle of the left femur resting upon a fixed support, the position being essentially the same as that adopted by Lombard[1].

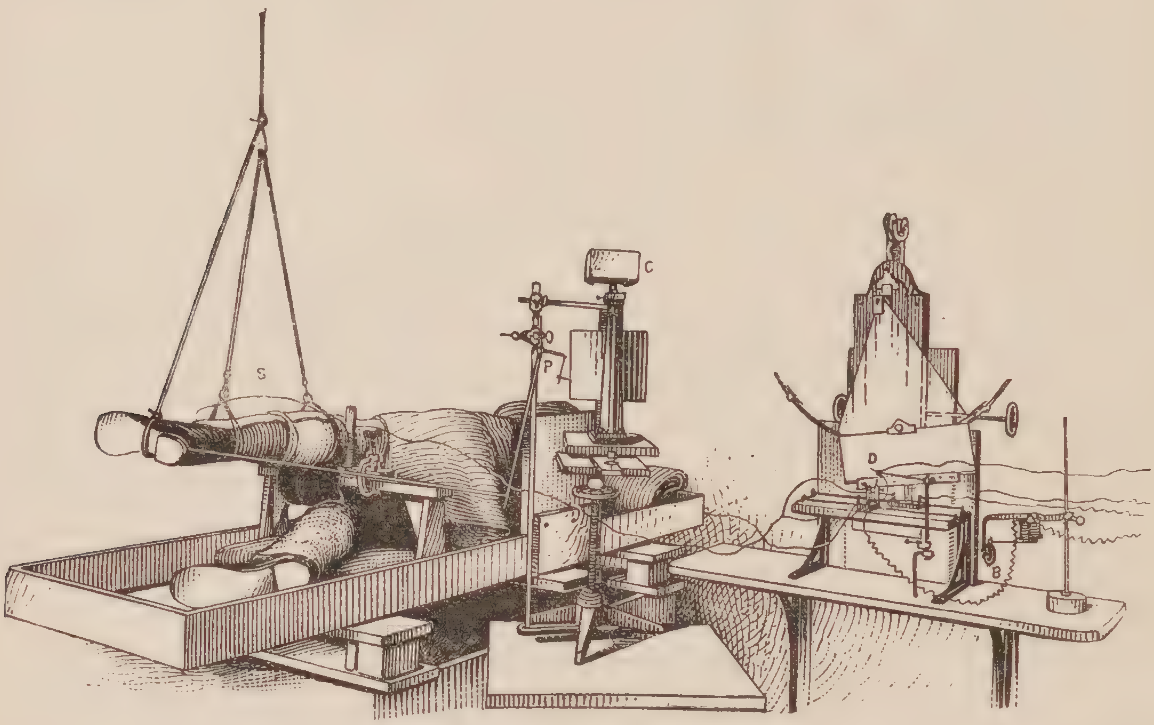

FIG. 2.

The lower leg was thus free to swing in a horizontal plane round the knee joint as a pivot, and its movement, reduced by a system of levers to one sixth of its extent, was recorded by a pen *P*, upon the smoked surface of a cylinder revolving once an hour[2]. Photographic reproductions of such records are given in Figs. 10, 11, 17, 18. To facilitate the adjustment of the apparatus and to secure greater uniformity in the blows the subject of the experiment wore trousers with the left leg removed so that only a single thickness of underclothing separated the skin and the hammer. The choice of the left leg in these experiments was quite accidental and due merely to the arrangement of the rooms in which the experiments were made.

[1] *Am. Journal of Med. Sciences*, Jan. 1887.

[2] For a description of this slowly revolving cylinder see This *Journal*, VI. 423.

In some of the earlier experiments on muscular reinforcement in which the time between the reinforcing act and the blow on the tendon was less than 0·5″, a small pendulum myograph[1] (Fig. 2, *D*) furnished with adjustable keys was used to break two electric circuits, one controlling the magnet holding up the hammer at the knee, and the other an electric bell *B*, a stroke of which on the breaking of the circuit was the signal for the reinforcing act.

It was soon found necessary to study the effect of much longer intervals, and for this purpose a pendulum, constructed on the principle

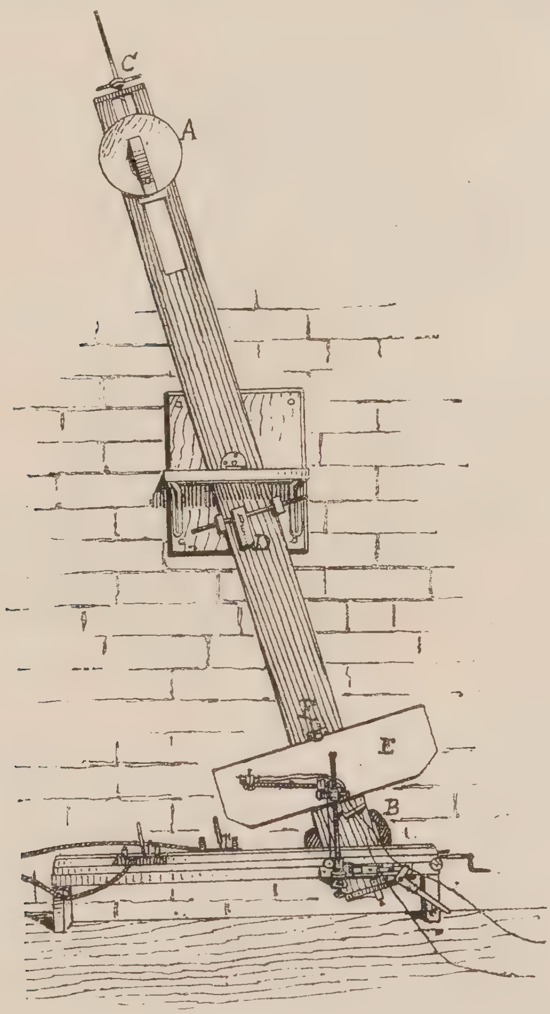

Fig. 3.

of the metronome and making a single vibration in 5″ was employed. Fig. 3 gives a general view of this piece of apparatus. It consists of a straight bar of wood 155 cm. long and 10 cm. broad turning on knife edges near its centre and bearing at its ends two discs of lead *A* and *B*, each weighing about 5 kilos. The upper disc is adjustable in a vertical slot by means of a screw *C*, so that the rate of vibration of the

[1] Invented and described by Dr J. J. Putnam, This *Journal*, II. 206.

pendulum may be regulated. A small lead weight *D*, adjustable on a horizontal rod, makes it possible to so balance the instrument that when at rest it shall remain absolutely vertical. A thin wooden board *E*, designed to carry a smoked card for receiving a record, was so arranged as to be readily attached to the pendulum by a spring catch *F*. The arrangement of this board as well as the construction of the lower part of the pendulum and of the keys which it opened in its movement can be best understood by reference to Fig. 4, which represents this portion of the apparatus on a larger scale. It will be noticed that the record board *E* bears upon its surface a set of lines radiating from the axis of

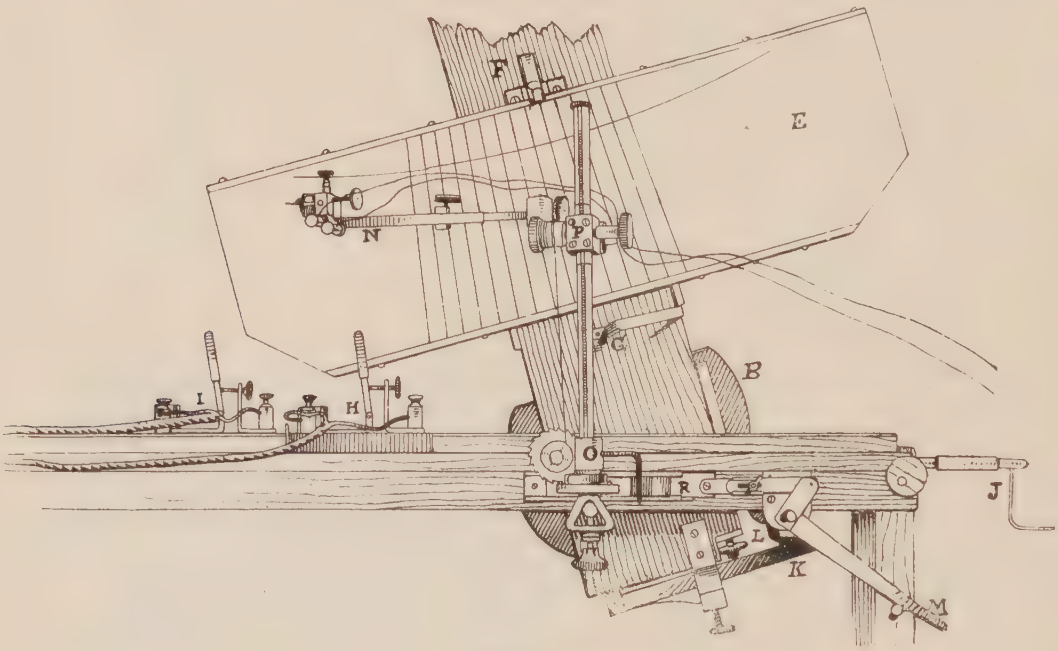

FIG. 4.

motion of the pendulum. The distance between these lines represents the motion of the pendulum in each tenth of a second during its vibration. They can be readily drawn upon any smoked card which is placed upon the record board by laying a ruler upon guiding lines engraved on the brass borders which hold the card upon the board. The position of these guiding lines has been determined by careful experiments with a standard tuning-fork and a Deprèz signal magnet.

Just below the record board above described a strip of brass *G* projects perpendicularly from the surface of the pendulum, being held firmly in this position by an angular brace. As the pendulum swings this strip strikes against and opens the two keys *H* and *I*, which are adjustable in parallel grooves on a horizontal board behind which the pendulum moves and which bears upon its surface a graduation in tenths of a second corresponding to that upon the record board. It is

thus possible by properly adjusting the keys to vary the interval of time between the opening of the two electric circuits to any desired extent. The adjustment of the keys takes place by means of rods with screw threads cut upon them running the whole length of the board and turning, by means of a crank, *J*, in a nut placed in the base of each of the keys. For more rapid adjustment the nut of one of the keys is split so that the key can be rapidly moved by the hand to any desired position and then more accurately adjusted by the crank.

If it is desired to change the position of the key rapidly and frequently the nut is removed, and the key, when moved by the hand, is made to communicate its motion by a string and pully, not shown in the figures, to a pen writing on the revolving cylinder below the knee-jerk record. The interval between the reinforcing act and the knee-jerk in each observation is thus automatically recorded. This form of automatic record is shown in the curves which are reproduced in Figs. 17, 18.

A noiseless opening of these keys is secured by drawing a piece of thin rubber tubing over the movable vertical portion which comes in contact with the brass strip *G*, and allowing it, when struck, to fall into a groove filled with a plug of cotton wool.

The device for starting the pendulum and catching it again when it has completed its double vibration is shown at *K*, *L*, *M*. A spring catch *K*, fastened to the lower end of the pendulum, when held by the tooth *L*, keeps the pendulum from swinging. This tooth is attached to one end of a rod running under the board carrying the keys, and turns on its axis in bearings fastened to the edges of the board. To the other end of the rod is attached a lever *M*, which when raised carries the tooth *L* and with it the catch *K* to the right. After the pendulum has thus been carried backward a few millimeters, the tooth *L* slips out from the catch *K*, and the pendulum is free to swing. The lever *M* is then lowered and the tooth brought back to its former position, where it receives and holds the pendulum on its return, for the latter, having begun its movement from a point a little more distant from the position of equilibrium, has sufficient momentum to carry the catch *K* over the tooth *L*.

In using the record board it is of course important that the recording pen should be in contact with the smoked surface only during the forward movement of the pendulum. This is accomplished by carrying the recording pen (a Deprèz magnet *N*) upon a holder *O*, the vertical portion of which is furnished with a rack and pinion

movement *P*, and is adjustable round its own axis, being carried by a spring (*Q* in Fig. 5) in such a direction as to remove the pen

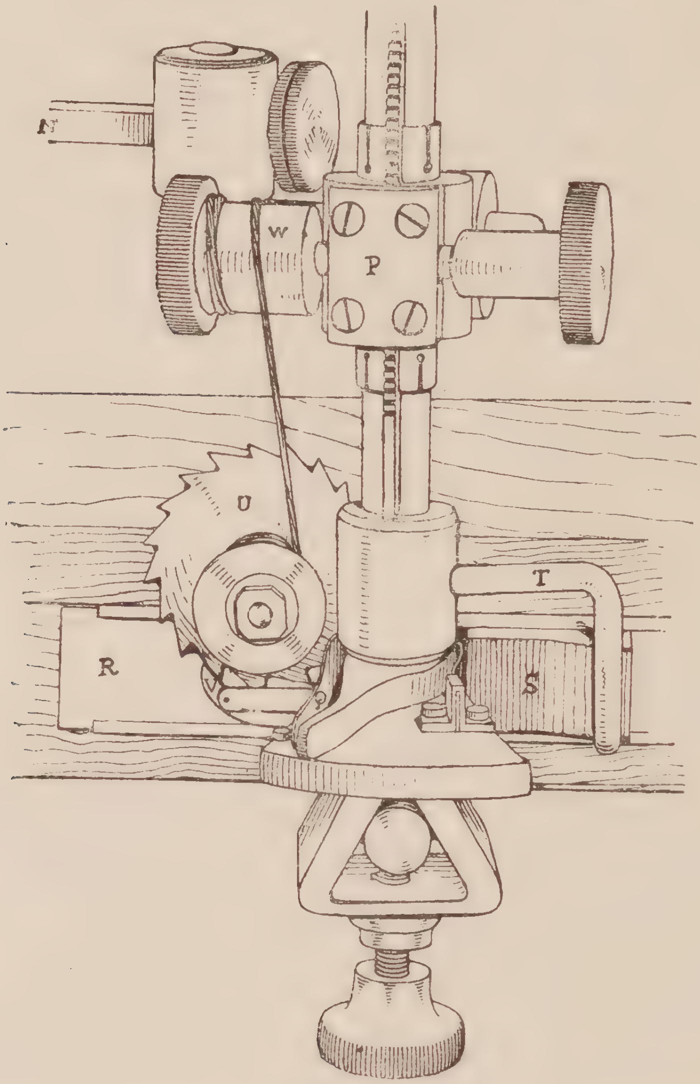

FIG. 5.

N from the smoked surface. Fig. 5 shews the mechanism by which the movement of the lever *M* is made to regulate this adjustment.

A strip of brass, *R*, moves horizontally in bearings fastened to the front edge of the key-board. By means of a connecting rod of adjustable length this strip may be attached to the lever *M*, as shown in Fig. 4, so that whenever the lever is raised and lowered the strip *R* is pushed to and fro in its bearings.

Fastened to the front of this strip is a block of hard rubber, *S*, having at one end a wedge-shaped form. Against the face of this block rests the vertical portion of a short brass rod *T*, bent at right angles and fastened securely to the base of the holder. The spring *Q* keeps the rod *T* pressed against the block *S*, and the latter therefore, as its wedge-shaped portion moves to and fro behind the rod *T*, gives to

the vertical portion of the holder a movement around its own axis. Thus when the lifting of the lever *M* starts the pendulum, as above described, the same movement applies the pen to the surface of the record board and, when the pendulum has completed its forward swing, the lowering of the lever, which brings the tooth *L* into a position to receive the catch *K*, also removes the pen from the record board and thus prevents the blurring of the tracing and any injury to the pen which would result from its remaining in position during the backward swing of the pendulum.

It is of course necessary, in a series of experiments, that the result of each observation shall be recorded on a separate line. This is accomplished automatically as follows. When the brass strip *R* is moved to the left in the manner above described, the rachet *V*, attached to it, is carried against the teeth of the spur wheel *U* and turns it a short distance round its axis. This movement winds up a thread on the axis of the spur wheel and unwinds it from a drum, *W*, on the axis of the rack and pinion movement. By this means the pinion and with it the recording pen is drawn downward a distance determined by the size of the drum *W*, relatively to that of the one on the axis of the wheel *U*. When the brass strip *R* is moved to the right, the rachet *V* slips past two teeth of the spur wheel and is ready for another movement.

It will thus be seen that the raising of the lever *M*, besides starting the pendulum, applies the pen to the surface of the record board and draws it down, so that the tracing is made upon a fresh portion of the smoked surface. Whenever normal knee-jerks are to be studied it is evidently unnecessary to use the record board on the pendulum. This recording apparatus is then disconnected by detaching the lever *M* from the brass strip *R*, until a reaction time or other evidence of a reinforcement is to be recorded. By this arrangement it became possible to make a clear record upon one card of all the reaction times, eighty or more in number, belonging to an experiment of an hour's duration with muscular reinforcements.

The following reinforcing acts were studied with reference to their effect upon the knee-jerk.

(1) A voluntary muscular movement.

(2) A sudden auditory stimulation.

(3) A sudden visual stimulation.

(4) A stimulation of the skin or mucous membrane by a sudden blast of air.

The voluntary muscular reinforcement was produced in response to

a stroke upon a small electric bell [Fig. 2, *B*] caused by the opening of one of the keys (which may be called the reinforcing key) by the swinging pendulum. The muscular act was a short vigorous clinching of the right hand upon a piece of wood, shaped somewhat like a tuning-fork, and furnished with metallic tips so adjusted that a slight pressure would bring them into contact, and thus close the same electric circuit that was opened when the bell was struck. As this circuit also included a Deprèz signal-magnet writing upon the record board of the pendulum, the reaction time of the individual was determined for each observation. The importance of these determinations will be seen in the discussion of the results.

The sudden auditory stimulation was produced by dropping a large paper torpedo near the head of the individual experimented on. The dropping was effected by placing the torpedo in a small pan turning on a hinge and held up by an electro-magnet controlled by a circuit containing the reinforcing key.

The visual reinforcement was produced by a flash of light caused by the sudden opening and closing of a stop-cock in a gas pipe supplying a burner in a lantern, the condensing lens of which was placed a few inches from the eyes of the person experimented on. A dark cloth surrounding the head and the lens of the lantern kept the individual in tolerably complete darkness except when the opening of the stop-cock and the sudden flashing up of the flame threw a momentary flood of light upon the eyes. The construction of this stop-cock is shown in Fig. 6. A base board, *A*, supports a straight piece of gas pipe furnished with two stop-cocks, *B* and *C*, and with tips, *D* and *E*, for the attachment of rubber tubing, supplying gas to the burner. An electro-magnet supported upon an adjustable board *G*, and controlled by a circuit containing the reinforcing key, holds against its poles the armature *H* which is borne upon one end of a horizontal arm, the other end of which is pivoted upon the lower end of a vertical rod firmly fixed above and in the prolongation of the axis of the stop-cock *B*. A spiral spring *I* coiled around this rod and bearing against the horizontal arm tends to press the armature away from the magnet. When, therefore, the reinforcing key is opened the armature flies around to the position represented in Fig. 6, where it is received upon a rubber pad borne upon a brass support fastened to the base board at *A*. (This pad and its support are for the sake of clearness omitted from the figure.) A stiff brass wire *K*, soldered to the stop-cock *B*; and passing through a hole in the horizontal arm, compels the stop-cock to follow the motions of the

armature *H*. When, therefore, the latter on the opening of the reinforcing key makes the above-described semicircular movement, the stop-cock *B* is momentarily opened and closed again. This causes a sudden flashing up of the flame of the burner in the lantern, which by a proper

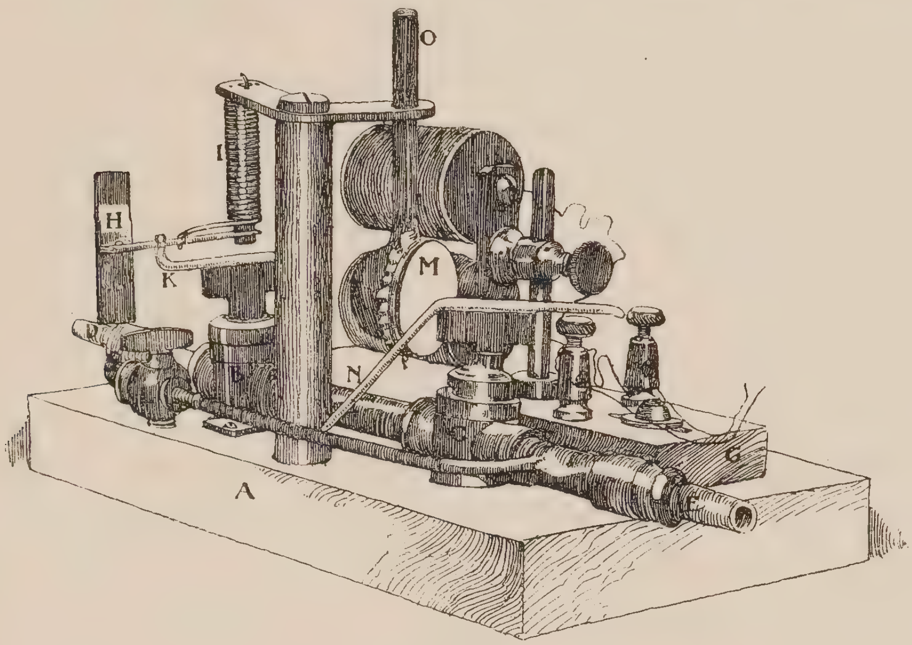

FIG. 6.

adjustment of the small side stop-cock, *L*, is never allowed to go entirely out even when the stop-cock *B* is closed. The stop-cock *C*, which is opened and closed by hand, serves to prevent the increased supply of gas, afforded by the momentary opening of *B*, from reaching the burner. When the stop-cock *C* is closed there is therefore no visual reinforcement, while all the other details of the experiment, including the slight noise accompanying the movement of the armature, remain unchanged. The experimenter can thus cause normal and reinforced knee-jerks to alternate with each other, either singly or in series, in any desired way. The error caused by the state of expectant attention of the individual experimented on is thus eliminated.

A stiff wire *N* fastened to the stop-cock *C*, serves as a handle to open and close it, and when the cock is open presses with its curved extremity upon the rubber membrane of the drum, *M*. This drum communicates by the tube *O*, and a rubber tube adapted to it with a Marey's recording drum writing on the revolving cylinder directly over the pen which records the extent of the knee-jerk. The tracing produced by the recording drum is therefore a broken line, the elevations of which correspond to those knee-jerks which receive a visual reinforcement, as shown in Figs. 17, 18.

This same arrangement of stop-cocks served to produce a stimulation of the skin or mucous membrane by a blast of air. This was effected by connecting the tip *D* with the reservoir of compressed air belonging to the artificial respiration apparatus of the laboratory, and the tip *E* with a small glass tube directed against the conjunctiva, the nasal mucous membrane, or any portion of the cutaneous surface.

It is evident that the various reinforcing acts above described must take place at varying intervals after the opening of the reinforcing key. For instance, the flash of light follows the opening of the key much more quickly than the sound of the torpedo which has to fall five or six feet before it explodes. Evidently therefore the space on the key-board between the reinforcing key and the knee-jerk key corresponding to any given interval of time between the reinforcing act and the blow upon the *ligamentum patellae* must vary with the nature of the reinforcement which is studied. The exact position of the reinforcing key which would cause the reinforcement to coincide in time with the blow on the knee when the knee-jerk key was set at zero, was in the earlier experiments ascertained by careful chronographic determinations of the time occupied by the descending hammer, the falling torpedo, &c. It was found however that the sensations of the individual experimented on were a sufficiently accurate guide for the determination of the zero point. In the later experiments, therefore, when a series of observations with any given kind of reinforcement was begun, the knee-jerk key was set at zero of the scale and the reinforcement key so adjusted that the reinforcing act (i.e. the sound, the flash or the blast) sensibly coincided with the blow on the knee. The reinforcing key was then kept in that position and the varying intervals produced by changes in the position of the knee-jerk key.

The results of the observations on the effect of the various reinforcing acts above enumerated on the extent of the knee-jerk will next be considered.

Muscular reinforcement.

The voluntary muscular movement, which formed the reinforcing act, was, as above described, a short vigorous clinching of the right hand upon a species of electric key by means of which the reaction time of the individual to the reinforcing signal was recorded.

As a reaction time always includes a centripetal, a central and a centrifugal portion, and as it was impossible to say in advance which portion of this nervous process would have the greatest influence upon

the knee-jerk, it was decided, in the earlier experiments, to regard the stroke of the bell as the zero point of the reinforcement, and when this point coincided in time with the blow of the hammer upon the *ligamentum patellae*, the interval between the reinforcement and the knee-jerk was called zero. The object of the investigation was, therefore, to ascertain how the extent of the knee-jerk would be affected by varying the interval of time at which the blow upon the *ligamentum patellae* followed the signal for reinforcement. No experiments were made in which the blow preceded the signal, since it would be difficult, after receiving the blow, to wait for the auditory signal before giving the reinforcing act.

Each experiment lasted, as a rule, about one hour. During this time several series of observations were made, each with a different interval between the bell-signal and the blow. Each series was divided into two portions, in the first of which there was no reinforcing act, and the knee-jerk was regarded as "normal," while in the second the individual responded to the bell-signal in the above-mentioned manner. It is perhaps worth mentioning that the bell-signal and every detail of the experiment, except the reinforcing act, were the same in both portions of each series. The difference between the average extent of the knee-jerk in the first and second portion of each series was called the special reinforcement for the interval corresponding to that series, and the difference between the extent of the knee-jerk in the second portion of each series and in the first portion of all the series in the same experiment was called the general reinforcement for the same interval.

Experiments with muscular reinforcement were made upon ten different individuals, of whom six presented similar general phenomena. In two cases no knee-jerk could be obtained with the apparatus in use, and it was not thought best to change the strength of the blow which in a large majority of cases was quite sufficient for the purposes of the investigation. In the remaining two cases the phenomena differed in a way which will be subsequently described from those which commonly presented themselves.

The general nature of the result obtained in the majority of cases may be best understood by an examination of the curve shown in Figure 7, constructed for a preliminary study and report from the record of 551 normal, and 624 reinforced knee-jerks in the same individual (B.). In this curve the abscissas represent the intervals between the bell signal and the blow, and the ordinates the difference

between the "normal" and the reinforced knee-jerks. Positive ordinates indicate an increase, negative a diminution of the knee-jerk. The figures at the left of the curve show in millimeters the absolute amount of increase or diminution. The dotted curve is the curve of special reinforcement; that is, it represents the average difference between the reinforced knee-jerk and the special normal of that series, while the full curve is the curve of general reinforcement representing the average difference between the reinforced knee-jerk and the general normal of the experiment. Both curves follow, it will be observed, the same general course, and show clearly that if the blow follows the signal at an interval not greater than 0·4″ the reinforcing act increases the extent of the knee-jerk. If the interval exceeds this amount a diminution of the knee-jerk results. If, however, the interval is prolonged to 1·7″ the reinforcing act is without effect upon the knee-jerk. To express the same conclusion in other words, we may say that, when by a brief act of volition the muscles of the forearm are innervated,

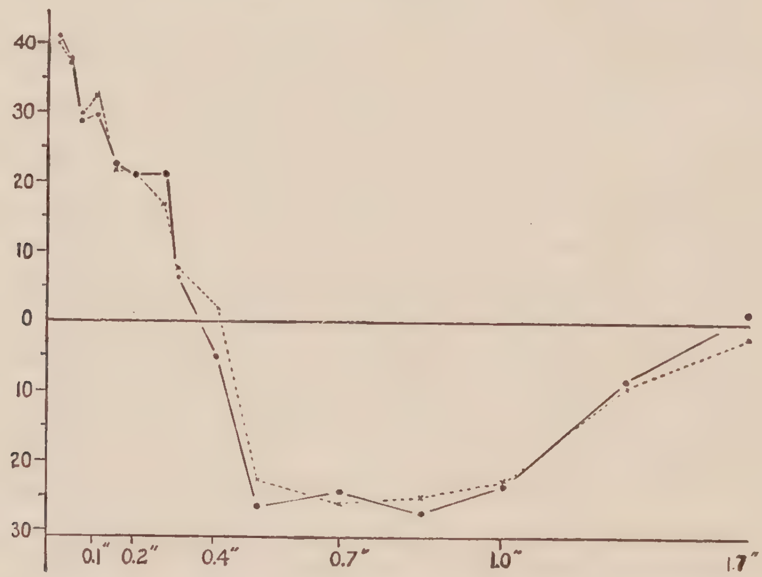

Fig. 7. Curves showing average muscular reinforcements arranged according to values of *i*.

the spinal cord is thrown into such a condition, that that portion of it which is concerned in the production of the knee-jerk is for a short time in a state of exalted activity, which is succeeded by a period of depression and then by a slow return to the normal state.

In the tabulation from which the above curve is constructed the observations are grouped according to the interval between the bell signal and the blow on the knee. Let this interval be indicated by the letter *i*, and let *r* stand for the reaction time. The interval of time between the beginning of the muscular reinforcing act and the blow on

the knee will then be expressed by $i-r$. Now as the value of r is always subject to considerable variation, it is evident that a curve obtained by grouping the observations according to the value of i does not, strictly speaking, express the reinforcing power of a muscular movement with reference to the varying intervals of time at which it preceded the blow on the knee.

It seemed therefore desirable to compare the above results with those to be obtained by tabulating the observations according to the value of $i-r$. This value was calculated for each observation by subtracting the reaction time as recorded on the pendulum from the interval as determined by the relative position of the keys. The extent

TABLE I.

Absolute values of reinforcement by muscular contraction.

B.			W.		
$i-r$	No. of observations.	D in mm.	$i-r$	No. of observations.	D in mm.
−0·16″	15	+40·0	−0·19″	35	+30·0
−0·11″	94	+37·3	−0·13″	265	+27·3
−0·07″	163	+33·0	−0·09″	528	+27·8
−0·03″	50	+26·4	−0·04″	259	+21·9
0·0″	37	+25·9	0·0	145	+11·1
+0·03″	29	+27·7	+0·04″	251	+9·2
+0·07″	19	+23·7	+0·09″	165	−23·5
			+0·13″	68	−44·6
+0·16″	9	+3·2	+0·19″	67	−46·3
+0·24″	19	+2·1	+0·25″	84	−36·0
+0·33″	21	−16·4	+0·34″	113	−39·4
			+0·44″	28	−48·1
+0·54″	12	−42·3	+0·57″	20	−38·7
			+0·66″	13	−37·9
			+0·77″	36	−24·1
+0·80″	13	−28·9	+0·83″	29	−26·8
			+0·98″	46	−23·4
			+1·15″	21	−22·5
			+1·27″	42	−12·3
+1·32″	10	−24·7	+1·33″	26	−12·1
			+1·47″	16	−11·7
+1·64″	3	−14·2			
+1·78″	19	−6·4	+1·77″	45	−5·0
			+2·25″	31	−5·7
Total No. of obser.	513			2333	

of the reinforced knee-jerk in each observation was then compared with the average value of all the normal knee-jerks in the experiment, and the difference, which may be called D, whether positive or negative, recorded. The observations were then arranged in groups according to approximate values of $i-r$, and the average value of D calculated for each group. Table I. shows this tabulation of the observations on the muscular reinforcement of two individuals, B. and W. The table is therefore divided into two vertical halves. In each half the first column gives the average value of $i-r$ in each successive group of observations. These values are of course negative in those cases in which $i<r$. This means that in certain cases the blow on the knee fell before the beginning of the muscular reinforcement, though it did not precede the signal to which the individual reacted. The second column gives the number of observations in each group, and the third the average value of D for each group. It will be seen for instance that in the case of B. 29 observations were made, in none of which the interval of time between the beginning of the muscular reinforcement and the blow on the knee differed materially from + 0·03″, and that the average extent of the knee-jerks in these cases exceeded the average of the normal or unreinforced knee-jerks in this experiment by 27·7 mm. The figures of Table I. have served for the construction of the curves of Fig. 8, in which the abcissas represent the values of $i-r$ and the

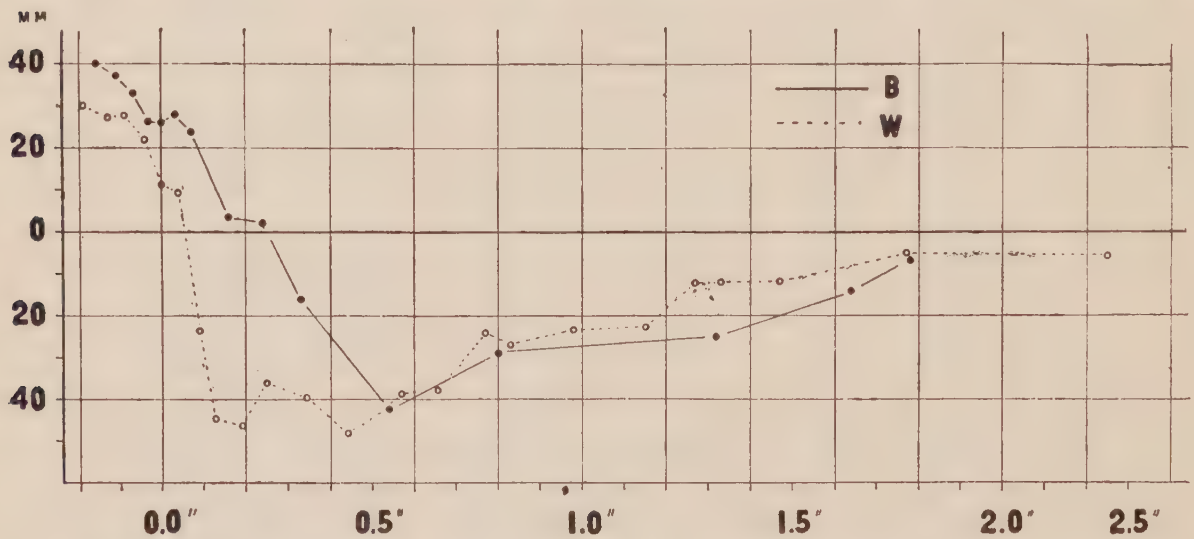

Fig. 8. Curves showing average muscular reinforcements arranged according to values of $i-r$. (Table I.)

ordinates those of D. The full curve shows the result of the observations on B., the dotted curve that of those on W. It will be observed that the general form of the full curve is the same as that of the full curve in Fig. 7, which represents the result of experiments on the same

individual tabulated according to values of i instead of $i-r$. In Fig. 7 the curve crosses the zero line at about 0·40″, in Fig. 8 at 0·25″, a difference of 0·15″. This difference should, of course, represent the value of r, and it may be regarded as confirmatory evidence of the accuracy of both these methods of tabulation that in the case of B. the value of r calculated from about 500 observations = 0·148″. In the case of W. the difference in the crossing points of the two curves corresponds to 0·175″, and the reaction time calculated from about 2000 observations is 0·184″.

In the course of this investigation, as observations accumulated, it became evident that the variation of the knee-jerk from day to day was too important to be neglected. In consequence of this variation a reinforcement of the knee-jerk by a given number of millimeters does

TABLE II.

Percentage values of reinforcement by muscular contraction.

Brief contraction. B_A.			Brief contraction. W.			Prolonged contraction. B_B.		
i	No. of observations.	*P*	*i*	No. of observations.	*P*	*i*	No. of observations.	*P*
0·0″	132	171 %	0·0″	377	153 %			
			0·02″	103	134 ,,			
0·04″	54	165 ,,	0·04″	30	138 ,,			
0·06″	54	147 ,,	0·06″	98	131 ,,			
0·10″	66	145 ,,	0·10″	340	142 ,,			
0·15″	43	141 ,,	0·15″	34	142 ,,			
0·20″	53	150 ,,	0·20″	441	133 ,,			
0·27″	42	146 ,,	0·24″	49	87 ,,			
0·30″	44	113 ,,	0·30″	181	51 ,,	0·30″	136	167 %
0·40″	41	97 ,,	0·40″	117	33 ,,	0·41″	30	126 ,,
0·51″	49	60 ,,	0·50″	106	44 ,,	0·51″	51	93 ,,
			0·60″	63	12 ,,	0·61″	10	58 ,,
0·70″	45	47 ,,	0·77″	29	33 ,,	0·71″	51	73 ,,
0·90″	47	39 ,,	1·00″	57	49 ,,	0·91″	41	69 ,,
1·09″	53	53 ,,	1·20″	43	64 ,,	1·11″	42	73 ,,
1·44″	30	73 ,,	1·49″	73	78 ,,	1·41″	20	97 ,,
1·71″	15	103 ,,	1·70″	13	83 ,,	1·71″	19	107 ,,
2·00″	20	96 ,,	2·00″	39	91 ,,			
			2·50″	26	88 ,,			
Total No. of obser.	788			2219			400	

not always represent the same proportionate increase of movement. It was therefore thought best to tabulate the above results in such a way as to show the percentage increase or diminution of movement for each value of i, the average value of all the normal knee-jerks in each experiment representing 100 %. Table II. shows, in the first and second divisions, this tabulation of the muscular reinforcements of B. and W. In each division the first column gives the average value of i in each group of observations, the second column the number of observations (i.e. reinforced knee-jerks) in each group, and the third column the average percentage value of the reinforced knee-jerks of each group with reference to the normal knee-jerk of the experiment. This value may be designated as P.

The third division of Table II. contains the results of some experiments made for the purpose of ascertaining how the reinforcement of the knee-jerk would be affected by the prolongation of the muscular act. For this purpose the individual experimented upon, instead of making the pressure on the recording instrument in his grasp as brief as possible, prolonged the effort till the blow fell upon the knee. It

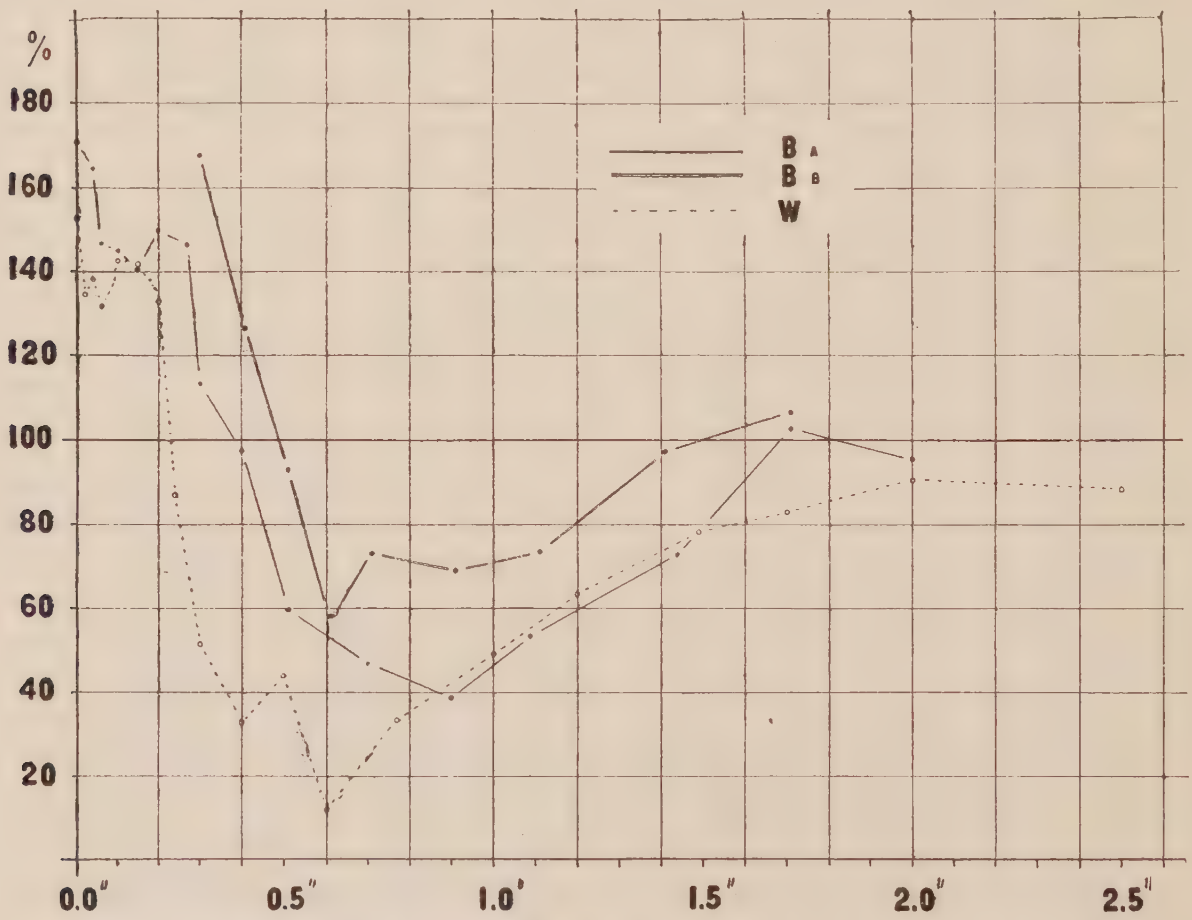

FIG. 9. Curves showing the percentage values of brief (B_A., W.) and of prolonged (B_B.) muscular reinforcement. (Table II.)

will be observed that in this third division of the table none of the values of i are less than 0·3″, for when the blow follows the reinforcing signal at a shorter interval there is obviously no appreciable difference between the shortest possible muscular act and one which is prolonged till the blow falls. The conclusions to be drawn from Table II. will be most readily understood from an examination of the curves in Fig. 9 which have been constructed from it. It will be noticed in the first place that the full and the dotted curves representing the short muscular reinforcements of B. and W. are similar to each other and of the same general character as those of Figs. 7 and 8. They indicate that when the blow on the knee is synchronous with the reinforcement signal the effect of the muscular act is to increase the knee-jerk 50—70 %. As the interval between the signal and the blow is increased the reinforcing power of the muscular act diminishes; the curves descend rapidly and cross the 100 % line at 0·38″ for B. and 0·23″ for W. This descent is in both cases interrupted by a temporary ascent, the cause of which is a matter of conjecture. Its position on the curve between 0·15″ and 0·20″ seems to indicate a possible connection with the voluntary motor impulse directed to the muscles of the arm. The inhibition of the knee-jerk, represented by that portion of the curve which is below the 100 % line, is more marked both in its amount and its duration in the case of W. than in that of B. A similar difference will be observed in the sensory reinforcements of these two individuals.

The curve representing the effect of a prolonged muscular reinforcement has the same general form as the other curves, but the value of all its ordinates is greater. That is, the prolongation of the muscular act tends to increase the positive reinforcement and diminish the inhibition of the knee-jerk. That the continuance of the muscular contraction does not alter the character of the curve seems to show that the influence of voluntary nerve-muscle activity upon the knee-jerk is more marked at the beginning than at any subsequent period of its duration. This conclusion must however be accepted with some reservation, since experiments of this sort were made only on a single individual and the number of observations, as shown in Table II., were comparatively few in number. We were deterred from extending these observations to other individuals by the difficulty of assuring ourselves that the muscular reinforcement was actually prolonged without interruption until the blow came.

These experiments on the knee-jerk fully confirmed the statements

of Lombard as to the great variability of the phenomenon, and the average values of both the normal and the reinforced movements, as given in the above tables, must, on account of the wide range of the observations from which they were calculated, be regarded only as rough approximations to the truth. Of the general proposition that with a short interval between the muscular act and the knee-jerk the latter is increased and with a longer interval diminished in extent, there can be no doubt whatever. Interesting confirmatory evidence of this is afforded in those cases in which the value of $i-r$ is suddenly reduced by the failure of the individual to react promptly to the signal. A series of greatly inhibited knee-jerks is thus interrupted by one of normal or even of exaggerated value[1].

The genuineness of the inhibition and its independence of any control by the will was demonstrated again and again by unexpected changes of the interval without the introduction of "normals" to familiarize the individual with the interval. The promptness with which the character of the knee-jerk changes under such conditions leaves little room for doubt concerning the reality of the phenomenon. For example: In an experiment of 6. vi. 1888 the average amount of the reinforced kick for 0·1″ and 0·2″ was 128 and 130 mm. respectively; the interval was changed to 0·4″ and then to 0·3″ and the kick fell at once to 13 and 15 mm. Similar results were obtained in other series of experiments in which the groups of "normals" were omitted for the reason above mentioned.

[1] The following figures may serve as an example of this effect:

Date.	*i.*	Average value of reinforced knee-jerks of the series.	Abnormally large reaction times.	Value of corresponding knee-jerk.
23. v. 1888	0·5″	15 mm.	0·412″	55 mm.
29. v. ,,	0·5″	15 ,,	0·410″	59 ,,
30. v. ,,	0·4″	19 ,,	0·354″	47 ,,
	0·3″	31 ,,	0·343″	70 ,,
31. v. ,,	0·6″	3 ,,	0·801″	66 ,,
			0·376″	37 ,,
4. vi. ,,	0·6″	7 ,,	0·643″	53 ,,
			0·606″	74 ,,
			0·491″	52 ,,
7. vi. ,,	0·4″	13 ,,	0·508″	144 ,,
	0·3″	9 ,,	0·349″	77 ,,
B.				
6. vi. 1888	0·9″	10 ,,	0·776″	52 ,,
	0·7″	14 ,,	0·516″	40 ,,
			0·614″	71 ,,

The following little table will serve to illustrate this point:

8. vi. 1888		19. vi. 1888				21. vi. 1888	
i	average reinforced knee-jerk	*i*	average reinforced knee-jerk	*i*	average reinforced knee-jerk	*i*	average reinforced knee-jerk
0·1″	126 mm.	0·4″	23 mm.	0·2″	58 mm.	0·1″	166 mm.
0·3″	42 „	0·2″	56 „	0·1″	84 „	0·4″	21 „
0·5″	17 „	0·1″	68 „	0·0″	118 „	0·3″	12 „
0·3″	40 „			0·3″	29 „	0·2″	116 „
0·2″	110 „	0·0″	103 „	0·1″	66 „	0·3″	21 „
		0·4″	11 „				
						0·3″	27 „
						0·2″	142 „

It is also worthy of notice that the order of the intervals was varied from day to day, and the negative reinforcement was shown to be really due to the interval employed and not dependent upon the duration of the experiment.

Fig. 10 is a full sized reproduction by a photographic process of two portions of the same curve. At the right are seen five normal (*N*) knee-jerks as given with an interval of 0·6″ between the keys; these are followed by ten knee-jerks reinforced by a voluntary muscular

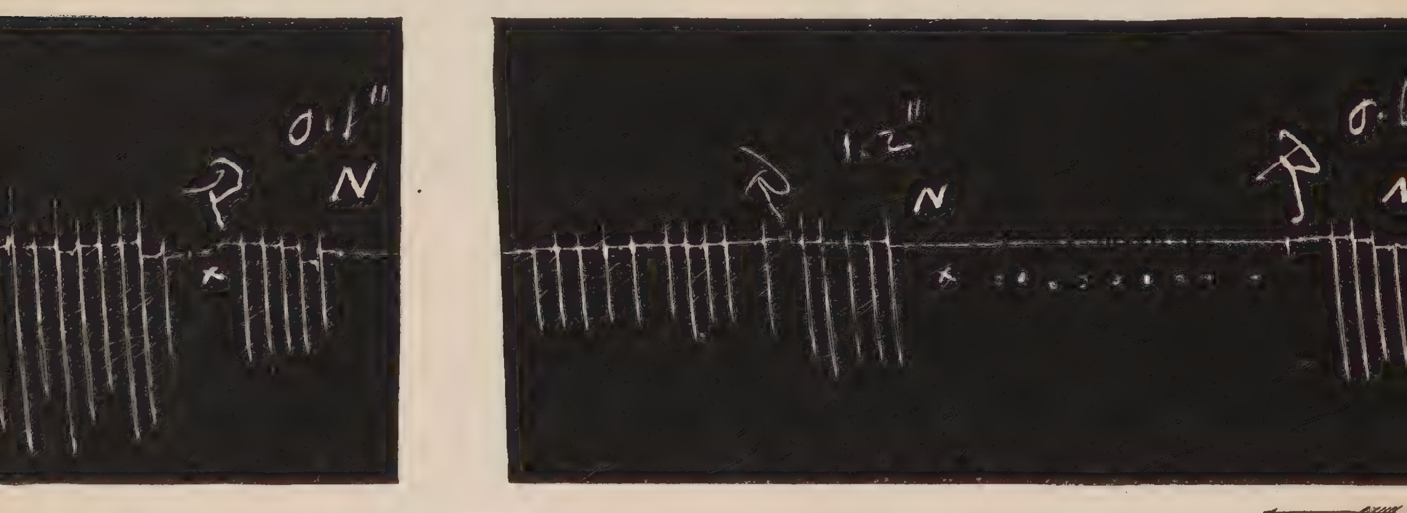

Fig. 10. Parts of same curve (full size) showing positive and negative reinforcement caused by muscular movements at various intervals.

movement, and so complete was the negative reinforcement that for nine of the records dots were required to recall the fact that this number of blows on the ligament had actually been given. The interval was then changed to 1·2″ and five normals taken succeeded

by ten reinforced kicks (*R*) which show an inhibition but much less than at 0·6″. At the left of the figure is seen a later portion of the same curve taken with an interval of 0·1″. The normals here are somewhat smaller than in the earlier part of the experiment, but the reinforcement as shown in the ten records is positive and distinct.

Fig. 11 is a reproduction of parts of another similar record of the full size of the original. Here as in Fig. 10 the negative reinforcement is distinct at the interval of 0·6″, the ten kicks following *R* being nearly reduced to zero. In the later part of the curve at the left it is seen that a return to a short interval (0·1″) renders the reinforcement

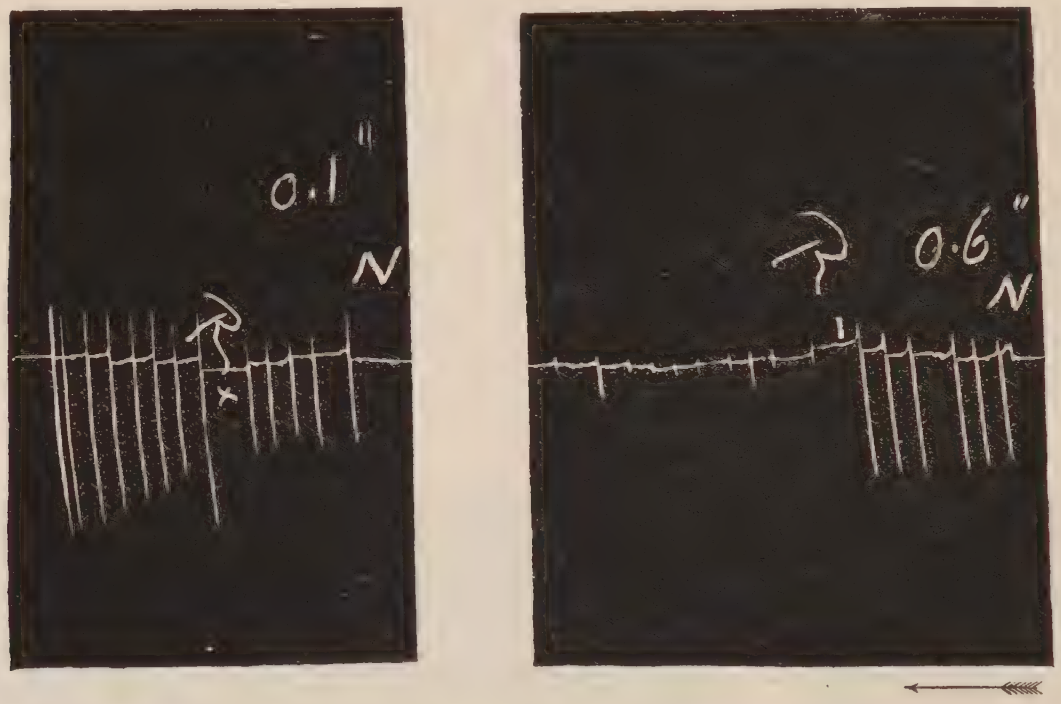

Fig. 11. Muscular reinforcement of the knee-jerk at different intervals in the same experiment.

positive again. Both of these curves show incidentally that the negative reinforcement is not the expression of fatigue due to the duration of the experiment.

There is, however, a great difference between individuals in respect to the extent to which the knee-jerk is reinforced or inhibited by a muscular movement. The fact that the tendency to inhibition is stronger in W. than in B. has already been alluded to. On the other hand, in two out of the ten individuals experimented on the effect of the muscular act was wholly positive, no value of *i* being found to give an inhibition.

It may therefore be said that in most of the cases examined the effect of a voluntary muscular movement was to increase the extent of a knee-jerk produced by a blow falling within 0·2—0·4″ of the beginning of the muscular contraction but to diminish the extent if the blow was longer delayed. If however the interval was prolonged to about 2·0″ no effect of the muscular movement upon the knee-jerk could be noticed.

Sensory Reinforcement.

A voluntary muscular movement in response to a signal is such a complicated nervous process that a study of the phenomena of reinforcement and inhibition under simpler conditions seemed desirable. Experiments were therefore undertaken with sensory stimulation of various sorts, the first effect studied being that of a sudden auditory impression produced at varying intervals of time before or slightly after the blow on the knee. The sound was that produced by the explosion

TABLE III.

Percentage values of reinforcement by auditory stimulus.

i	B.		W.		C.	
	No. of observations.	*P*	No. of observations.	*P*	No. of observations.	*P*
– 0·3″	44	104 °/₀	62	100 °/₀		
– 0·2″	60	92 „	108	101 „	26	84 °/₀
– 0·1″	67	104 „	128	108 „	36	110 „
0·0″	71	139 „	136	125 „	46	146 „
+ 0·1″	72	142 „	148	124 „	40	158 „
+ 0·2″	87	168 „	165	132 „	43	223 „
+ 0·3″	100	179 „	146	122 „	51	278 „
+ 0·4″	93	153 „	147	119 „	44	216 „
+ 0·5″	88	123 „	118	116 „		
+ 0·6″			120	117 „	41	223 „
+ 0·7″	64	118 „	127	112 „		
+ 0·8″					38	184 „
+ 1·0″	63	116 „	117	114 „	51	167 „
+ 1·2″	50	120 „				
+ 1·4″					45	159 „
+ 1·5″	52	115 „	81	97 „		
+ 2·0″	49	122 „	114	97 „	23	110 „
+ 3·0″			41	96 „		
Total No. of obser.	960		1758		484	

of a torpedo made of coarse sand wrapped in paper weighing about 13 grams and containing a small charge of fulminating powder. The torpedoes were specially made for these experiments with a view to securing uniform intensity of sound, a result which was, however, not fully attained. The torpedoes were dropped on the opening of the reinforcing key in the manner above described (p. 34).

Table III. gives the result of experiments on auditory reinforcements on B. and W. and also on a third individual (C.). It will be observed that the reinforcements were in nearly all cases positive in their character, a fact which is further illustrated by the curves of Fig. 12 which have been constructed from the values of i and P for B. and W. as given in Table III. The curves lie almost wholly above the 100 % line and in this respect differ very decidedly from those representing the reinforcement by muscular contraction as shown in Table II. and Fig. 9.

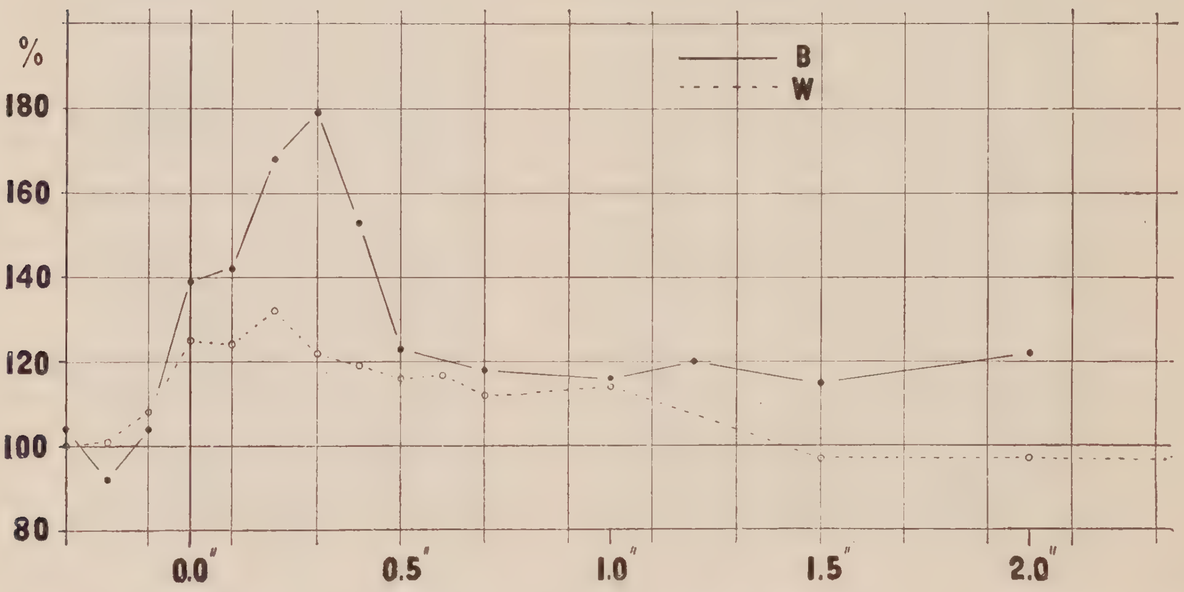

FIG. 12. Curves showing the reinforcing influence of auditory impressions. (Table III.)

It will be noticed that the reinforcement is much stronger in the case of B. than in that of W., while in that of C. it is so much stronger than in either of the others that the curve could not readily be drawn on the same coordinates.

The next form of sensory reinforcement which was studied was that produced by a sudden flash of light caused by the opening of a stop-cock in a gas pipe in the manner above described (pp. 34, 35). The results of one series of observations on B., two on W. and one on C., are given in Table IV. It will be noticed that the values of P in the observations on B. and C. are nearly all above 100 %, while in the case of W. they are below

TABLE IV.

Percentage values of reinforcement by visual stimulus.

i	B.		$W_{(A)}$.		$W_{(B)}$.		C.	
	No. of observations.	P	No. of observations.	P	No. of observations.	P	No. of observations.	P
− 0·2″	102	93 %	102	115 %	22	91 %	66	92 %
− 0·1″	112	106 „	90	114 „	24	107 „	70	99 „
0·0″	113	128 „	93	139 „	44	118 „	84	178 „
+ 0·1″	70	146 „	105	120 „	56	120 „	69	209 „
+ 0·2″	78	151 „	102	121 „	22	83 „	71	188 „
+ 0·3″	81	152 „	92	102 „	24	82 „	80	163 „
+ 0·4″	71	110 „	96	96 „	19	55 „	68	135 „
+ 0·5″	113	93 „	78	88 „	20	65 „	82	130 „
+ 0·6″	61	100 „			30	57 „	71	155 „
+ 0·7″	106	113 „	91	72 „	17	75 „	66	145 „
+ 0·8″	71	102 „	80	64 „	8	70 „	73	134 „
+ 0·9″	61	97 „						
+ 1·0″	86	94 „	73	80 „	34	74 „	83	109 „
+ 1·2″	84	108 „	48	73 „			89	115 „
+ 1·4″	68	100 „					59	109 „
+ 1·5″			45	72 „	13	67 „	74	117 „
+ 1·6″							56	107 „
+ 1·7″	70	106 „						
+ 2·0″			35	83 „	11	84 „	48	90 „
+ 2·2″	79	113 „						
+ 2·5″							40	108 „
Total No. of obser.	1426		1130		344		1249	

that limit for all values of i exceeding 0·3″. In other words, a visual stimulus produces on W. an effect similar to that of a muscular contraction, while in the cases of B. and C. the effect is like that of an auditory stimulus. That this difference is not an accidental one is shown by the fact that it appears in both series of observations on W. which were taken at an interval of three months[1]. In Fig. 13 are

[1] The observations tabulated under $W_{(A)}$. in Tables IV. and V. were made in the autumn of 1888 and seemed at the time to suggest that the muscular movement involved was probably a more potent influence upon the knee-jerk than the sensory stimulation of the flash or blast. During the succeeding three months W. was absent from the laboratory, and the corresponding experiments were made upon B. and C. with the results given in the Tables. These results were so unexpectedly different from those which the previous agreement of B. and W. had rendered probable, that new series were taken in the

given two curves, constructed from the values of P in the columns headed B. and $W_{(A)}$., which show very clearly the difference between the two individuals in this respect.

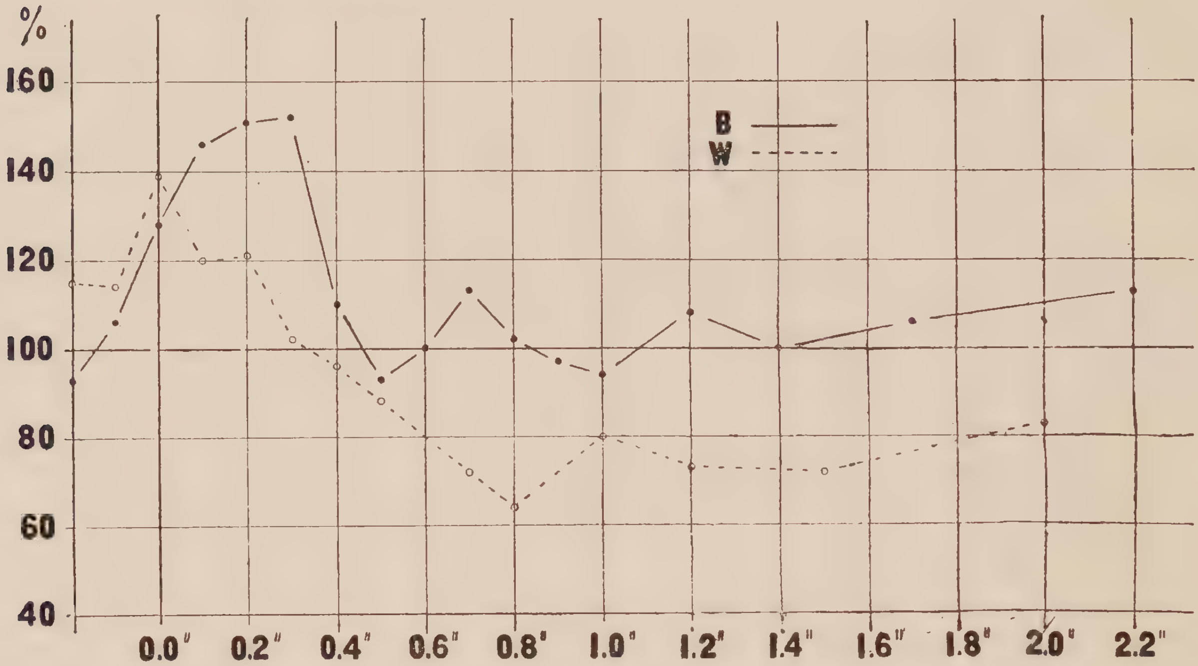

FIG. 13. Curves showing the influence of a visual stimulus on the knee-jerk. (Table IV.)

In searching for a possible explanation of the more pronounced inhibitory effect of a visual as compared with that of an auditory stimulus (a phenomenon to be noticed in B., C., and W. alike, though only in the case of the latter giving values of P below 100 %), the possibility of a movement of winking combining its effect with that of a sensory stimulation suggested itself. It was found in fact that such a movement nearly always occurred when the brilliant flash of light fell upon the eyes and, indeed, it would seem difficult to avoid such a movement except by a forcible contraction of antagonistic muscles which would of itself produce the effect of a muscular reinforcement.

To test this hypothesis a form of stimulation was chosen which would produce a reflex wink with a minimum of sensory stimulus. A blast of air directed upon the conjunctiva seemed to be best adapted for this purpose, and the same electro-magnetic stop-cock which served to produce the flash of light was, as above described, made to deliver a sudden blast of air through a tube attached to an ophthalmoscopic mirror-holder and directed against the conjunctival mucous membrane.

spring of 1889 [those tabulated as $W_{(B)}$] to test the accuracy of the older series. As these later sets were in substantial accord with the earlier ones (both in detail and when grouped) it was deemed unnecessary to make the observations as numerous as before.

Table V. and Fig. 14 show the result of the observations made in this way upon the same individuals who served for the former determinations.

TABLE V.

Percentage values of reinforcement by stimulus of conjunctiva.

i	B. No. of observations.	B. P	$W_{(A)}$. No. of observations.	$W_{(A)}$. P	$W_{(B)}$. No. of observations.	$W_{(B)}$. P	C. No. of observations.	C. P
– 0·4″	51	98 %						
– 0·3″	48	95 ,,						
– 0·2″	105	139 ,,	58	118 %	50	103 %	86	60 %
– 0·1″	80	139 ,,	75	104 ,,	54	109 ,,	90	101 ,,
0·0″	76	163 ,,	72	107 ,,	61	110 ,,	128	218 ,,
+ 0·1″	83	170 ,,	72	116 ,,	48	113 ,,	128	361 ,,
+ 0·2″	77	159 ,,	69	111 ,,	31	79 ,,	141	307 ,,
+ 0·3″	106	113 ,,	80	92 ,,	12	54 ,,	102	328 ,,
+ 0·4″	102	104 ,,	67	83 ,,	30	63 ,,	117	255 ,,
+ 0·5″	80	108 ,,	77	85 ,,	27	64 ,,	140	281 ,,
+ 0·6″	76	107 ,,	75	78 ,,	50	61 ,,		
+ 0·7″	78	103 ,,	61	84 ,,	30	54 ,,	165	240 ,,
+ 0·8″	97	101 ,,			19	48 ,,		
+ 1·0″	93	103 ,,	53	65 ,,	71	49 ,,	133	148 ,,
+ 1·2″	87	104 ,,						
+ 1·5″	91	102 ,,	65	76 ,,	35	54 ,,	71	94 ,,
+ 2·0″	110	114 ,,	44	83 ,,	42	63 ,,	48	128 ,,
+ 2·5″							59	77 ,,
Total No. of obser.	1440		868		560		1408	

$W_{(A)}$ is the autumn series and $W_{(B)}$ the confirmatory series taken in the spring.

It will be noticed that in the case of W., who happened to be the first individual experimented upon, the values of P are decidedly less than in the experiments with a visual stimulus, a result quite in harmony with the hypothesis that the inhibitory effect of motor activity in winking is the cause of the difference between the result of auditory stimulation, as shown in Table III., and that of visual stimulation, as shown in Table IV. This hypothesis, however, derived no support from the observations upon B. and C. (for in these cases the values of P were found to be larger than in the experiments with visual stimulation), and was, moreover, shown to be quite untenable by the result of

experiments upon W., in which the air blast was directed against the nasal mucous membrane. Here the sensory stimulus was quite insignificant and there was no perceptible muscular reaction as in the case

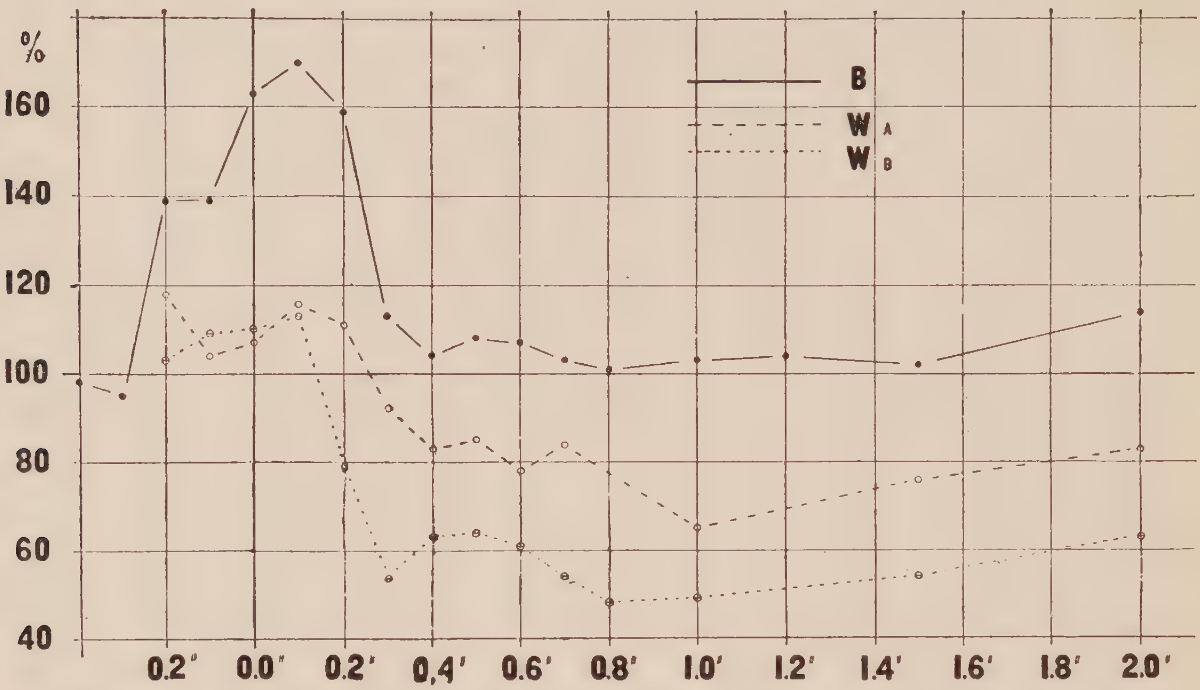

Fig. 14. Curves showing the influence of stimulating the conjunctiva by a blast of air. (Table V.)

of conjunctival stimulation; yet the values of P as shown in the first section of Table VI. and graphically in Fig. 15 are quite as low as in the corresponding experiments in Table V.

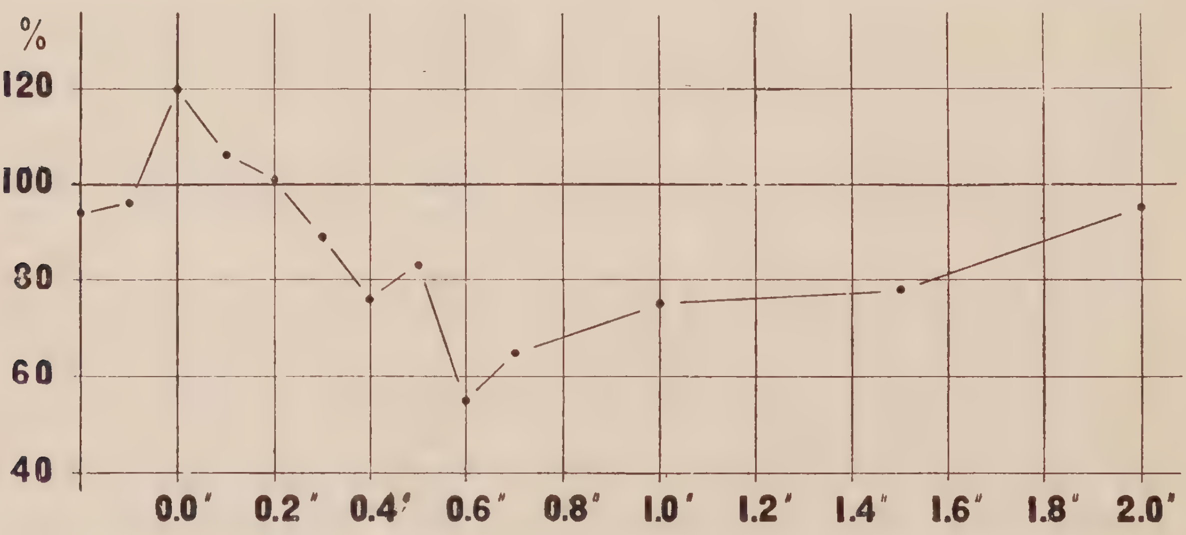

Fig. 15. Curve of knee-jerks modified by stimulating the mucous membrane of the nose by a blast of air. (Table VI.)

The study of the influence of sensory stimulations was then continued by directing the blast of air on to the external skin. Such a stimulus

might seem too slight to produce any effect, but the well-defined results obtained by the subjectively slight stimulation of the nasal mucous membrane led us to hope for some positive results, and the experiments fully justified the expectation. These experiments, made only upon C. and W., are collated in the second part of Table VI. and the curves

TABLE VI.

Percentage values of reinforcement of knee-jerks by stimulation of:

i	I. Nasal mucous membrane.		II. Skin of neck.			
	W.		C.		W.	
	No. of observations.	P	No. of observations.	P	No. of observations.	P
− 0·2″	62	94 %	35	80 %	73	97 %
− 0·1″	55	96 ,,	45	93 ,,	67	101 ,,
0·0″	68	120 ,,	69	115 ,,	157	105 ,,
+ 0·1″	70	106 ,,	88	156 ,,	145	104 ,,
+ 0·2″	73	101 ,,	110	192 ,,	119	119 ,,
+ 0·3″	81	89 ,,	77	165 ,,	111	96 ,,
+ 0·4″	77	76 ,,	62	171 ,,	118	70 ,,
+ 0·5″	63	83 ,,	49	144 ,,	99	71 ,,
+ 0·6″	74	55 ,,	43	132 ,,	117	62 ,,
+ 0·7″	62	65 ,,	90	117 ,,	123	61 ,,
+ 0·8″			109	114 ,,	121	60 ,,
+ 1·0″	52	75 ,,	77	116 ,,	129	59 ,,
+ 1·2″			55	104 ,,		
+ 1·5″	42	78 ,,	69	76 ,,	76	66 ,,
+ 2·0″	41	95 ,,	63	96 ,,	66	79 ,,
Total No. of obser.	820		1041		1521	

are shown in Fig. 16. The stimulus was the blast of air directed to the skin of the neck in the median line at the level of the fourth or fifth vertebra; the tube was held in position by the strap and clamp of the ophthalmoscopic mirror fastened to the head in a reversed position. As before, W. shows a distinct positive reinforcement for the shorter intervals and a more marked negative reinforcement for the longer ones with a gradual return to the normal, while with C. the effect is almost wholly that of a positive reinforcement of the knee-jerk. The genuineness of these results was tested not merely by the very large number of observations of which the table is made up, but also and more especially

in the case of W. by frequent and quite unexpected changes of interval. The promptness with which a positive reinforcement became negative for an increase in the interval so slight that W. could not readily

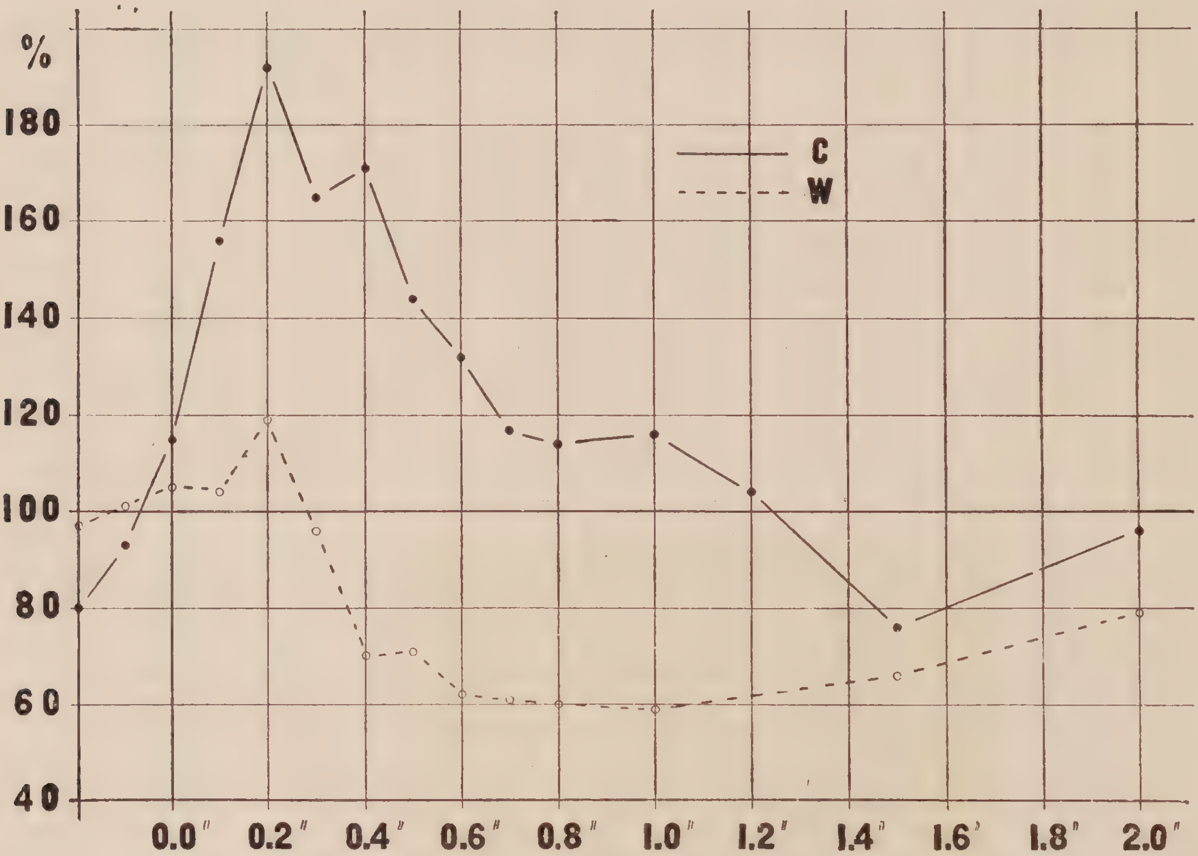

FIG. 16. Curves showing the percentage value of knee-jerks modified by a blast of air directed upon the skin of the neck. (Table VI. II.)

perceive it, or a negative reinforcement was replaced by a positive one for an equally unexpected and slight lessening of the interval, would seem to preclude the explanation that expectancy, or any preconceived idea as to what would happen, had influenced the result, and this demonstration is most complete for just those intervals (0·2″—0·4″) where the results are most marked[1].

These sudden changes of interval are only possible, when made in any considerable number, by using the automatic recording apparatus, which notes the exact position of the reinforcing key and also the fact that the recorded knee-jerk was or was not associated with a sensory stimulation and leaves almost nothing for the experimenter to neglect. This apparatus has already been described (p. 33), but its value will be

[1] In a recent experiment the neck blast and torpedo were used alternately during the same hour, and the results were in general like those of the curves already given. The difference in the effect of the two kinds of stimulation came out clearly at an interval of 0·6″, where the kicks influenced by the torpedo averaged 114 %, while those in which the air blast was in play only averaged 76 % of the normal of the experiment.

better understood by an examination of the fragments of records photographically reproduced in Figs. 17 and 18. The former (Fig. 17) shows the negatively reinforced knee-jerks of W. produced by the "neck blast". The curve is of full size and shows clearly the work of the apparatus. At the bottom is a series of parallel lines drawn on the paper before the experiment begins by the pen which is connected with the reinforcing key by a string and giving the position of the pen for the more important positions of the key. At the top of the figure is a broken line which shows the position of the stop-cock C and its rod N in Fig. 6 (p. 35). When the stop-cock is closed, that is when no sensory reinforcement by flash of light or blast of air can be given, the record is a straight line, and we know that the corresponding knee-jerks are to be counted as "normals". When however the experimenter decides to bring a reinforcement into play and opens the

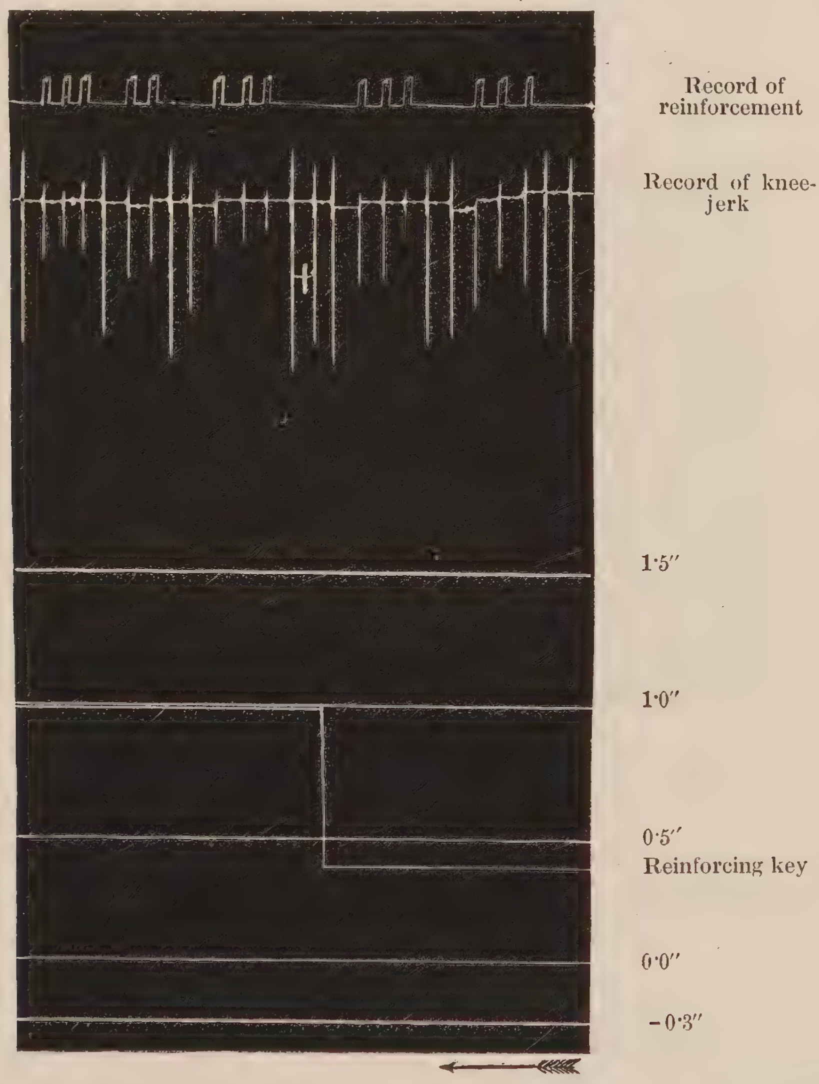

FIG. 17. Portion of record (full size) showing the automatic method of noting position of reinforcing key and use of reinforcement in an experiment with the "neck-blast". The knee-jerks are $\frac{1}{6}$ the actual size.

stop-cock *C* the upper arm of the rod *N* is pressed upon the rubber drum *M* and the lever of the recording drum, attached to this as already described, is moved, and the line of the "reinforcement record" is broken by a change of level; when the knee-jerk has been produced the experimenter closes the stop-cock again before a new observation is made. In this way each break or tooth in the record of reinforcement means that a stimulation was given and that the corresponding knee-jerk is to be regarded as "reinforced". Fig. 18 is a part of a similar "neck blast" curve photographically reduced in size.

In Fig. 17 the reinforcing key was first set at 0·4″ and then changed to 1·0″, but the experimenter failed to set it exactly and his oversight is accurately recorded. The curve shows the negative reinforcement obtained for W. at these intervals and also the ready alternation of "normals" and "reinforcements" attainable by the apparatus. In Fig. 18 the reinforcing key stood in the beginning at 0·1″ but was changed in this part of the experiment to 1·2″, 0·4″, and then to 0·8″.

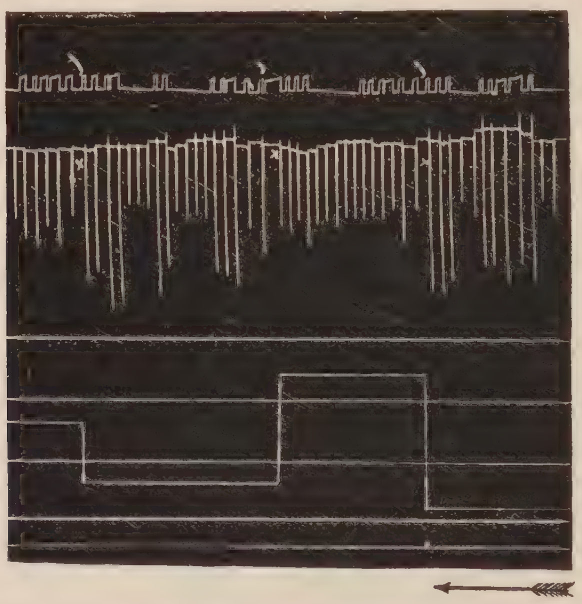

Fig. 18. Portion of another record (*c*) like Fig. 17 but photographically reduced.

All these changes were made in the midst of series of reinforcements, and the lessening of these with the increase of the interval is very distinct. The "teeth" in the "record of reinforcement" are much less regular than in Fig. 17, because the experimenter several times forgot to close the stop-cock promptly after the record had been made. For convenience of enumeration a little mark (+) is made among the knee-jerk records at the time of changing the interval.

Having obtained such definite effects by stimulating the external

skin it seemed desirable to test the influence of locality in this connection. At present we are only able to report the results obtained with one person by directing the air blast upon the skin of the knee on the inner side between the internal condyle and the *ligamentum patellae.* The stimulus in this case corresponds very nearly in locality to that of the hammer itself so far as the external skin is concerned. The details are noted in Table VII. and presented graphically in Fig. 19. Although the number of observations is large it will be seen that no marked difference is shown between the two knees. The reinforcement apparently produces little or no change in the size of the knee-jerk in these experiments until the interval between the blast and the blow has reached 0·3″, when the reinforcement becomes markedly negative, and then the curve has about the same form as in the other experiments with W. Evidently other tracts of skin ought to be

TABLE VII.

Percentage values of reinforcement of knee-jerks by stimulation of skin of knee (W.).

i	B left.		A right.	
	No of observations.	*P*	No. of observations.	*P*
– 0·2″	46	102 %	20	91 %
– 0·1″	69	102 „	37	94 „
0·0″	75	103 „	70	95 „
+ 0·1″	74	98 „	60	99 „
+ 0·2″	90	104 „	71	93 „
+ 0·3″	78	83 „	59	85 „
+ 0·4″	78	67 „	61	65 „
+ 0·5″	76	71 „	58	57 „
+ 0·6″	80	67 „	55	64 „
+ 0·7″	69	53 „	58	60 „
+ 0·8″	73	53 „	52	58 „
+ 1·0″	71	57 „	63	56 „
+ 1·5″	67	64 „	40	56 „
+ 2·0″	54	66 „	37	73 „
+ 2·5″	32	63 „		
+ 3·0″	7	71 „		
Total No. of obser.	1039		741	

tested and the observations extended to other individuals, but this we are at present unable to do. As a tentative explanation of the disappearance in Fig. 19 of the distinct but small positive reinforcement seen in Figs. 15 and 16, while the negative reinforcement is about the

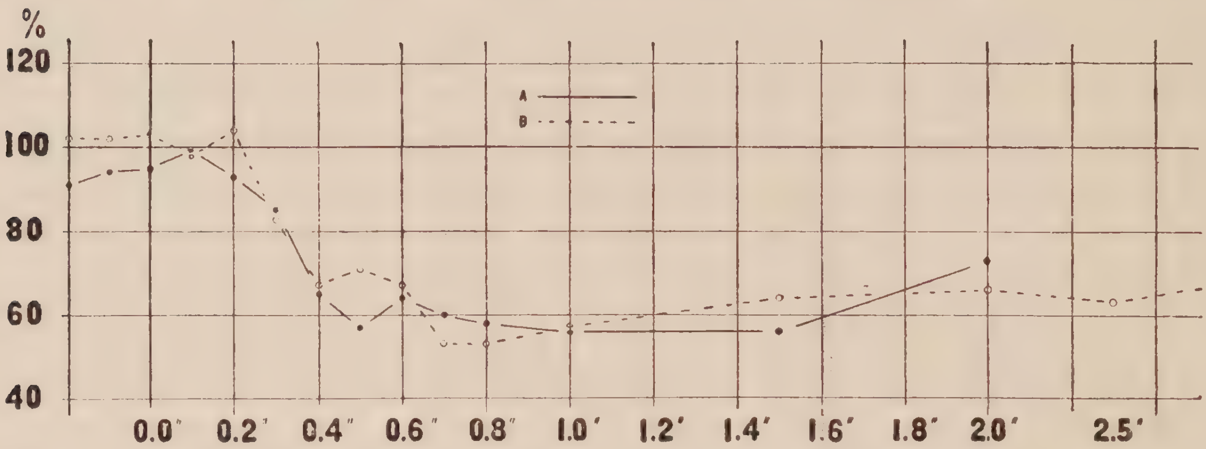

FIG. 19. Curve of knee-jerks as affected by a blast of air upon the knee (W.). A right, B left knee. (Table VII.)

same in the three curves both as to time and as to quantity, it should be remembered that the "knee blast" merely adds a slight skin stimulation at a point where the blow of the hammer itself produces a very marked stimulation. When these nearly correspond in time no very definite increase of the knee-jerk, i.e. a positive reinforcement, is to be expected; when, however, the interval is increased the extra skin stimulation although slight begins to manifest an independent influence, and the result is in general the same as when the blast of air reached the nose or the neck. It may be however that the length or some other quality of the nervous path to be traversed is also of importance. Such a possibility is suggested by the fact that in some few experiments upon the left knee (that is blast and blow being upon the same knee) there seemed to be a very marked positive reinforcement as in the case of other sensory stimulations, while this did not clearly appear in the experiments with the blast directed to the right knee.

Effect of Sleep.

The monotonous character of the experiments was often found to produce a decided tendency to sleep in the individual experimented on. To counteract this tendency and to insure a certain degree of attention to the phenomena the subject of the experiment was required to declare after each knee-jerk whether or not a sensory stimulus (i.e.

sound, flash, &c.) had been perceived or, in other words, whether the knee-jerk was normal or reinforced[1].

In spite of this precaution the tendency to sleep was sometimes quite irresistible, and in eight or nine cases the experiment was continued after the subject had yielded to it and was sleeping soundly. It was then found that the knee-jerks, both normal and reinforced, grew gradually smaller, and, when sleep was profound, disappeared entirely, the blow upon the knee being absolutely without effect[2]. This result is not what might have been expected from our knowledge of the effect of sleep on the ordinary cutaneous reflexes, e.g. that produced by tickling the sole of the foot. Whether this can be regarded as an argument against the reflex character of the knee-jerk, or whether we have here an essential difference between deep and superficial reflexes, are questions to be decided by future investigations[3].

[1] In the experiments made with muscular reinforcements the arrangement of the apparatus obliged us to record "normal" or "reinforced" in distinct groups, although the interval might be unexpectedly changed without the knowledge of the person experimented upon. Here too the voluntary reinforcing act kept the attention fixed and drowsiness did not readily manifest itself. In the experiments with sensory reinforcements, on the other hand, the character of the knee-jerk (whether it was to be "normal" or "reinforced") was quite unknown to the subject of the experiment, and even the interval was changed frequently and unexpectedly although a certain amount of grouping was necessary.

[2] Lombard (*Am. Jr. Psychol.* 1887, I. p. 49) mentions the diminution of the knee-jerk "when quiet, or even a condition closely resembling sleep, had crept on," but he does not seem to have observed the absolute disappearance of the knee-jerk in sleep.

[3] In a few experiments this drowsiness was so evident that the observations of the entire series were rejected, it being quite impracticable to pick out those periods where the attention to the signals was unperverted. The cases where actual sleep occurred and the experiment was continued are unfortunately too few to permit any trustworthy inferences; in fact the observations in this connection were altogether too unsystematically made to allow us at present to say more than that the knee-jerk in response to a light blow upon the *ligamentum patellae* certainly disappears during sleep and again reappears with its full value when the individual is once more thoroughly awake. One of the most interesting series in illustration of this point was obtained while experimenting on W. to determine the importance of the bell signal in connection with the voluntary muscular reinforcement. At the beginning of the experiment the normals, taken in the usual manner, had an average of 83 mm. The blows upon the tendon were then continued without any warning sound whatever; the size of the movement gradually lessened and became almost 0 as W. apparently fell asleep, the entire group of 43 observations having an average of only 34 mm. W. was then awakened for a short time and the knee-jerk immediately rose again to 72 mm. The bell signal was then once more omitted, and the movement again grew smaller and became absolutely 0 as W. once more slept; after 32 observations (with an average of 32 mm.) the bell was again used and the movement increased immediately to 75 mm. The bell was once more omitted, and the average movement fell very rapidly to 23 mm. (for 25 observations) as sleep came on, and this was so profound that when the bell was again employed the effect was so slight and transitory

General Conclusions.

As the general result of the observations recorded in this paper the following conclusions may be formulated.

(1) In the majority of individuals experimented upon a voluntary muscular contraction occurring simultaneously with the blow upon the knee increases the extent of the knee-jerk, but with the prolongation of the interval between the reinforcement signal and the blow this effect is reversed; the knee-jerk becomes much reduced in extent and may even entirely disappear. With a still further prolongation of the interval the knee-jerk gradually returns to its normal value. The interval at which the effect changes from positive to negative varies with different individuals from 0·22″ to 0·6″. The interval at which the knee-jerk returns to its normal value is 1·7″—2·5″. In two individuals

that the next 18 observations (with "bell") had an average of only 17 mm. W. was then completely awakened and a few experiments with muscular reinforcement were made, giving results like those already discussed, and then the series was completed by taking 26 "normals" with an average of 65 mm.

This disappearance of the knee-jerk is even more evident in other observations made on W., although the number is usually much smaller. It is indeed by no means easy to be quite sure just where the sleep begins, although the failure to define the character of the blow suggests its approach and sometimes there is confirmatory evidence of an audible character. Even before real sleep has come the diminution of the knee-jerk is very marked, as may be seen in the "normals" of these two experiments where the air blast was directed upon the knee:—

	1 *May*, 1889	7 *May*, 1889
1. Awake	25 obs. averaging 59 mm.	18 obs. averaging 70 mm.
2. "Sleepy"	19 ,, ,, 23 ,,	22 ,, ,, 22 ,,
3. Awake (?)	18 ,, ,, 53 ,,	
4. "Asleep"	27* ,, ,, 5 ,,	13 ,, ,, 2 ,,
5. Awake	25 ,, ,, 59 ,,	21 ,, ,, 73 ,,

* 10 of these were absolutely = 0.

The "reinforced" knee-jerk is in general in these cases larger than the corresponding normals but 60 to 100 % smaller than corresponding reinforced kicks made while wide awake. The "reinforced" knee-jerk may also be reduced to 0 through sleep. This is certainly true when the blast of air is directed to the skin about the knee, and seems to be true even when the torpedo is used for purposes of reinforcement. It also seems from such few records as are available for this comparison that the normal knee-jerk disappears rather more easily and sooner than that which is associated with some reinforcing stimulus. Obviously the length of the interval, the kind of stimulus, and the force of the blow as well as the duration of the sleep itself should be considered in this connection, and for this purpose our material is at present altogether inadequate. We hope to find time to examine this interesting question with more care.

the effect of muscular contraction on the extent of the knee-jerk was wholly positive[1].

(2) The effect of a sudden auditory stimulus on the extent of the knee-jerk was, in the three subjects of experiment, almost wholly positive, though great individual differences were observed. The maximum effect was produced when the interval between the sound and the blow was 0·2″—0·3″.

(3) The effect of a sudden visual stimulus upon the extent of the knee-jerk was with two of the three subjects of experiment almost wholly positive, the maximum being reached when the interval between the flash and the blow was 0·1″—0·3″. With the third individual a positive phase having its maximum when the interval was zero gave place rapidly to a negative phase reaching its maximum at 0·4″—0·8″.

(4) The effect of a sudden stimulus of the conjunctiva by an air blast was in general similar to that of a visual stimulus, except that the positive phase in all three individuals reached its maximum when the interval between the blast and the blow was 0·1″, and the negative phase in the individual who manifested this phenomenon had its maximum at 0·8″—1·0″.

(5) The effect of a sudden stimulus of the nasal mucous membrane by an air blast was, in the only individual upon whom experiments of this sort were made, similar to that of stimulation of the conjunctiva. The greatest positive reinforcement was with an interval of 0·0″ and was slightly larger than that obtained from the same individual by stimulating the conjunctiva. The negative phase was most pronounced at 0·6″ and was as marked as with any sensory stimulus employed.

[1] In view of the fact that a muscular reinforcement has been used by clinical observers for some years in testing the patellar tendon reflex, it may seem strange that so distinct an influence of the interval should have remained unnoticed for such a length of time. Some observers (e.g. Mitchell and Lewis, and Lombard) remark that "to get the full effect of the reënforcement, the blow must be delivered at just the right moment after the reënforcing act," but this evidently refers only to variations of the positive reinforcement, the negative reinforcement, or inhibition, having escaped attention. The explanation appears to be this: In common clinical testing the muscular movement of reinforcement is made in response to some command or signal of the experimenter which probably also acts as his own signal for giving the blow upon the tendon. As the reaction times of the two persons concerned are usually approximately the same, it follows that the blow and the muscular movement of reinforcement will nearly coincide or differ only by an interval at which a positive reinforcement may be expected. If by any chance the interval happens to be such as to produce a negative reinforcement the examining physician probably considers this due to some failure of his blow or to some irregularity in the reinforcing act of the patient, and the influence of the interval, which may be the real cause of apparent failure, escapes detection.

(6) The stimulation of the skin of the neck brought out individual differences very similar to those observed in the experiments with the flash or the stimulation of the conjunctiva. One individual manifested a reinforcement of the knee-jerk which was almost altogether positive, while the other had as usual a positive and a negative reinforcement, the former being greatest at 0·2″, the latter at 0·6″—1·0″.

(7) When the blast of air was directed to the skin of the knee the knee-jerk was not distinctly affected until the interval reached 0·2—0·3″, at which point, in the only individual experimented upon, a distinct negative reinforcement appeared becoming most marked at 0·5—0·7″.

It will be observed that the three individuals upon whom most of the experiments were performed showed constant differences in their mode of reacting to the reinforcing stimulus. Whatever the nature of the stimulus the tendency to inhibition of the knee-jerk was more strongly marked in W. than in B., while in the case of C. the positive reinforcement was much more pronounced than in either of the other two individuals. That such individual differences should exist will surprise no one who has conducted experiments upon the nervous system of man and the higher animals, but their interpretation must be postponed till observations have been made upon a much larger number of persons.

The fact that the activity of a given portion of the nervous system may, in one individual increase, and in another diminish, the activity of a neighbouring portion, suggests that we are dealing here with fundamental differences of nervous organization, and it seems not impossible that observations of this sort may throw light upon those peculiarities of the central nervous system which determine what we now rather vaguely designate as the temperament or disposition of the individual.

The total number of observations from which the above conclusions have been deduced is rather more than 42000. The ordinates in most of the curves represent average values of 70—100 reinforced knee-jerks which, in their various series, have been compared with about the same number of normal kicks. Yet the broken character of the curves expressing the final results shows clearly that the number of observations, large as it is, requires to be much increased before the conclusions which are drawn can claim any high degree of accuracy. General statements, such as those above laid down, are all that can at present be safely made.

The great variability of the knee-jerk under normal circumstances, of course accounts for these irregularities. It has been clearly shown by Lombard that almost every form of mental activity has its effect

upon the extent of the knee-jerk, and the variations in the mental state of the subjects of the experiments, who were sometimes attentive and interested and sometimes indifferent, sleepy, or abstracted, were quite sufficient to cause the differences which were observed in successive knee-jerks under apparently identical conditions. It is doubtless impracticable to eliminate these sources of error altogether. It was our constant endeavour to conduct each experiment in such a manner as to remove all external disturbances, so that in long series of many observations the uncontrollable influence of the mental state or other factors should affect the general result as little as possible. If such uncertainty attaches to the determination of the physiological variations of the knee-jerk, great caution would appear to be required in establishing any conclusions about the influence of disease upon the same phenomenon. The conditions of the ordinary clinical test are hardly adapted to remove doubt as to the validity of results depending not merely upon few observations but also upon the state of mind of a patient vaguely disturbed as to what is to happen to him or even frightened about himself.

The continual variations of the knee-jerk caused by the ever changing psychical condition of the individual suggested the possibility that the phenomenon would prove to be more uniform in persons of enfeebled mental activity, and that conclusions might therefore be drawn from a smaller number of observations. Experiments were, consequently, tried upon two patients suffering from dementia, whom Dr T. W. Fisher, superintendent of the Boston (Mass.) Lunatic Hospital, kindly brought to the laboratory. The results were, however, no more satisfactory than with persons of intelligence, and the practical difficulties of making observations on patients of this sort were so great that the experiment was abandoned. It would seem therefore that the accurate formulation of the laws governing the physiological modifications of the knee-jerk must depend upon a still more extensive collection of observations on normal healthy individuals.

In conclusion we desire to express our obligation to Miss Lucy Ellis, through whose generosity means were provided for securing the services of an accountant, whose whole time for eight months was devoted to tabulating and averaging the enormous mass of experimental data relating to the effect of sensory reinforcements. Our thanks are also due to Dr Russell Sturgis and to Messrs Abbot, Coggeshall, Fitz, Pearson and Perry, students of medicine, who kindly volunteered to become subjects for our experiments.

Postscript. It ought to be added that Lombard[1] has shown that "occasionally the flexors, as well as the extensors, of the knee are seen to contract in response to the blow on the ligamentum patellae." He considers this flexor contraction as of reflex origin and not the result of mechanical stimulation. The form of record used in our experiments does not permit an examination of them with reference to Lombard's views. We may however profitably inquire whether the inhibition met with in our experiments may not be due to active contraction of the flexors and not to lessened activity of the extensor muscles. The character of the knee-jerk under these varying conditions is not favourable to such an explanation. The positively reinforced knee-jerk is commonly quicker and more jerky than the normal knee-jerk, while the negatively reinforced (or inhibited) knee-jerk has usually lost the jerky character altogether and is not merely a quick kick whose length is lessened by opposition. At least such is the feeling at the time of the experiment, and such a difference in the movements can be readily observed although our method does not record it. Numerous observations noted on a rapidly revolving drum would probably determine it. Were the inhibition due to the increased activity of antagonistic muscles it would seem probable that we should often have not merely the reduction of the knee-jerk to zero (i.e. complete inhibition), but also a negative knee-jerk, or flexion, at those intervals where the negative reinforcement is found. Such a negative kick is most unusual in our observations, so rare, indeed, that we must consider it quite accidental. Then too the gradual and regular return to the normal as seen in our curves illustrating the influence of voluntary muscular reinforcement would seem to be at variance with the assumption that the action of the flexors was the inhibiting cause. But before these points can be definitely settled the time relations of the response of the flexors must be more clearly made out. On this most important point Lombard's article throws little light, and it must not be forgotten that he himself speaks of the flexor contractions as only "occasionally" occurring in his experiments.

[1] "On the nature of the knee-jerk." This *Journal*, vol. x., p. 122.

Extracted from
The American Journal of the Medical Sciences for May, 1890.

A CASE OF FRACTURE OF THE TEMPORAL BONE, WITH REMARKS ON TREPHINING FOR FRACTURE OF THE BASE OF THE SKULL.

By J. Collins Warren, M.D.,
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The object of reporting this case is to call attention to the advantage of drainage as an aid to the process of repair after fracture of the base. Until quite recently this grave lesion has been regarded as beyond the pale of surgical interference. The latest literature shows, however, that hospital surgeons are endeavoring to apply the principles of antiseptic surgery to this almost inaccessible region, and the recent advances in brain surgery encourage the hope that more attention will be paid to the lesions of the different fossæ of the skull and their contents, and that valuable suggestions may from time to time be forthcoming as to the management of the complications to which they give rise.

T. K. L., seventeen years of age, a tall and slender man, was riding in the saddle about dusk of the afternoon of October 20, 1889, and had separated from his companions, having ridden rapidly ahead of them when the accident occurred. According to the statement of a passer-by, he was observed turning a corner in the road at a moderate pace, when suddenly horse and rider were seen struggling in the dust. The horse soon regained his feet and ran away. It is supposed that as the corner was turned the horse shied suddenly, throwing the rider and falling upon him.

When the patient was approached he was found to be comatose and bleeding profusely from the nose and left ear; a narrow strip of brain substance was found upon the sleeve of his coat. The accident occurred about 6 P.M. On my arrival (about 10 P.M. the same evening) I found the patient comatose with heavy breathing. There was, apparently, no paralysis of any kind; the pupils were normal and reacted to light; the pulse was of good strength and about 100 beats to the minute; hemorrhage from the nose had ceased, but the patient had vomited blood an hour or so before. Blood was still oozing from the left ear, and on close inspection two fragments of brain substance, each about the size of a pea, were found at the external auditory meatus. There was a contusion on the forehead just above the glabella, forming quite a prominent tumor. Careful examination of the scalp failed to reveal any depression of the bone. There was a boggy swelling of the integuments of the left temporal bone above and behind the ear, and a faint depression about two inches above the external meatus, which, however, was thought not to be due to displacement of bone. Pressure on these parts produced a fresh flow of

blood from the ear, the flow ceasing between the intervals of examination. No external wound was visible anywhere. The presence of brain in the discharge from the ear, the contusion of the neighboring scalp, and the profound coma pointed to serious injury at the base of the brain and in the left temporal bone.

The evidence of grave injury at this spot seemed so pronounced, and the prognosis of the case seemed so unfavorable, that although no depressed fragments of bone were felt, it seemed justifiable to depart from the ordinary rule of non-interference in fracture of the base and to make an exploratory incision for the purpose of arresting hemorrhage, affording drainage, or removing clots and fragments of bone.

The operation was performed shortly after midnight—that is, about six hours after the accident—in the presence of Drs. Hall Curtis, S. J. Mixter, and John Homans, 2d., the latter having charge of the case subsequently. The scalp on this side of the head having been shaved, a curved incision was made, starting from the auriculo-bregmatic line at a point about two and one-half inches above the external auditory meatus and returning to the level of the meatus. The flap thus made, when reflected forward, exposed the posterior half of the squamous portion of the temporal bone. A nearly vertical fracture was found at about the juncture of the middle and posterior thirds of that bone: at its upper end the fracture terminated in several radiating cracks, about one-half inch in length each, which crossed the squamous suture and involved the adjacent parietal bone; it ran downward and slightly forward to the external auditory meatus, where it could be seen running along the wall of the auditory canal. (Fig. 1.) There was no depression of the bone, but the edges of the cleft were slightly sprung at the lower portion. Considerable dark-colored fluid blood with small clots welled up from beneath on slight pressure on the bone. A one-inch trephine was placed directly over the line of fracture a short distance above the meatus, and a button of bone removed, the lower edge of which was nearly on a level with the floor of the middle fossa. The dura was found torn at this point, and the finger being introduced, could follow the fracture along the petrous portion of the temporal bone, and on being turned upward found its way to the depth of the second kunckle into a lacerated wound of the middle lobe. There were a few small clots only removed from the wound, but a considerable quantity of fluid blood escaped which had evidently been confined beneath the bone; an arterial branch had been torn at this point, as shown by the groove in the button removed, and bled freely, but was easily controlled by pressure. The decalcified bone drainage-tubes which I had brought with me were found to be too much softened for use, and a strip of bichloride gauze was accordingly pushed gently along the floor of the skull, and a number of strands of catgut tied together were pushed up into the lacerated brain tissue; a small strip of gauze was placed in the auditory canal. A dressing of bichloride gauze and absorbent cotton, fortified by a folded towel wrung out of the diluted solution, was applied and held firmly in place by a figure-of-eight bandage. Antiseptic precautions were observed throughout the operation.

Immediately after the operation the patient was cold and pale and

the pulse weak; the coma remained as before; urine was passed involuntarily. During the night the dressing being stained, was reinforced. The temperature the next morning was 100.8° F., but fell to normal on the morning of the second day; he then appeared stronger. The dressing, on being changed, was found saturated with bloody serum. As it was quite a voluminous dressing, the amount of cerebro-spinal fluid and bloody discharge which it had absorbed must have been considerable. The drains were not disturbed and an iodoform gauze and absorbent

FIG. 1.

Line of the fracture through the squamous portion of the temporal bone. The dotted line shows the point at which the trephine was placed.

cotton dressing was applied. A strip of iodoform gauze was placed in the auditory canal, which at each dressing was carefully syringed out with sublimate solution, 1:5000. The temperature rose on the evening of the second day to 100.9°, the highest point reached, and fell steadily from this point to normal on the sixth day. On the fourth day the dressing was changed again: the catgut drain came away and the gauze was removed, and was replaced by a small strip of iodoform gauze inserted just within the margin of the cranial cavity; all the scalp stitches except one were removed. This dressing was allowed to remain three days, and on the 27th, just a week after the accident, all stitches and the gauze drains had been removed. During this period coma had been complete, but on this day, for the first time, the patient opened his eyes and recognized

his mother. The expected subconjunctival ecchymosis did not occur, it being evident that the effusion had had ample opportunity to escape through the drain established.

Careful attention was paid to the condition of the mucous membrane surrounding the opening of the Eustachian canal. A small amount of iodoform was blown into the left nostril with an "insufflator" once daily, and the nostril and pharynx were sprayed once or twice a day with a four per cent. solution of boracic acid. The hygiene of the mouth was also carefully attended to. It was hoped that in this way septic infection of clots in the middle ear from the side of the pharynx might be prevented. On the tenth day the gauze drain, which had been pushed less deeply into the wound, was removed altogether, and the granulating wound healed completely a few days later. A dressing of iodoform gauze was continued over the auditory meatus, which was syringed out with a sublimate wash every few days.

The mental condition of the patient during the the second week underwent little change. Beyond moaning and crying in a childish way, and staring with a puzzled expression when disturbed, there was little to indicate a knowledge of his surroundings. In the third week there was a decided improvement, the patient being able to recognize and name individuals about him and to remember me after an interval of several days. With returning consciousness a marked degree of sensory aphasia showed itself.

The temperature during this period was slightly subnormal and the pulse rose rapidly to 115 on any excitement. Sleep and appetite were, however, excellent. About this time it was noticed that the tongue, when protruded, curved slightly to the left, and the muscles of the right side of the face were not quite so powerfully contracted as those of the left. Beyond this, however, no sign of paralysis was oberved anywhere. There was a slow but steady improvement in all symptoms from this time on. A statement written out by his nurse on December 6th, about six weeks after the accident, gives a good idea of his mental condition at that time:

"In language he improves and generally can easily be understood, though there are many words whose meaning he has forgotten. 'Hilarity' had to be repeated or explained several times. . . . He will sometimes talk in French or German, but cannot explain or translate it. In trying to write the shortest or simplest words he cannot remember what letters to use. . . . He often says, or asks for, things opposite to what he means; for instance, he will say, 'myself and the other girls.' In some ways his senses are keen, while in others he is very child-like."

It should be mentioned in this connection that the patient had been a brilliant scholar.

On December 8th the ear was examined by Dr. Clarence J. Blake, who made the following report: "The walls of the canal are but slightly reddened; there is very little discharge from the ear, and the hearing is gratifyingly good, as the tuning-fork which he heard sixty-five seconds in the sound ear is heard between thirty-five and forty seconds in the left ear. By bone conduction the fork is heard better in the left ear. In this ear also the hearing for high tones remains as good as could be expected under the existing conditions, including rupture of the membrana tympani. The condition of the ear indicates fracture of the superior wall of the

external canal longitudinally and rupture of the membrana tympani, but no injury to the labyrinth."

There is little to be said about the further progress of the case, except that the mental improvement was rapid during the month of December, and by January 1st the aphasia had almost entirely disappeared. Under date of March 2d Dr. John Homans, 2d., writes me: "The tongue is still protruded slightly to the left; there are some hesitancy and stammering in the speech and an incapability of continuous mental exertion—that is, he cannot read aloud for more than half an hour without mispronouncing badly and having slight pain in the head, but in all these defects there is a steady, though slow, improvement."

On February 4th Dr. Blake writes: "To-day young L. has no discharge from the ear, the granulations have disappeared, the membrana tympani is intact, but somewhat opaque, from the thickening of the inner coat, the Eustachian tube is free, and the hearing after catheterization is for a Politzer acoumeter $\frac{65}{180}$."

The question of diagnosis is of importance in this case, as it is, of course, desirable to determine definitely whether we have a "fracture of the base" to deal with. So far as the appearances observed within the ear and the complete restoration of the hearing serve as evidence, it would appear that there had been a widely spreading fracture of the superior wall of the bony external auditory canal, stopping at the inner end of the canal. But such an assumption is highly improbable when we consider the anatomical peculiarities of the temporal bone and the structures which lie in the direct line of the fracture. The petrous portion of the bone is, it is true, largely composed of dense bony tissues, but the bony covering of the middle ear, which lies in the anterior portion of the petrous portion and near its junction with the squamous portion, is extremely thin. There are other structures which tend to make the bone particularly weak at this point.

Passing a probe through the Eustachian tube and the auditory canal of a dry specimen, and looking at the under surface of such a bone, we see that the probe marks a line of division between the squamous portion on the one side and the mastoid and petrous portions on the other. Along this line lie the canals for the Eustachian tube and tensor tympani muscle, the external auditory canal, and the Glaserian fissure. The carotid canal, although not on this exact line, is quite near it, and weakens this portion of the petrous bone. A fracture descending from the squamous portion and passing along the wall of the external auditory canal, parallel with its long axis, would, if continued onward in the same line, pass through the structures just mentioned; and, moreover, piercing the membrane of Schrapnell would pass in front of the apparatus for hearing. It is true that rupture of this membrane implies the opening of the tympanic cavity, but the membrana tympani is placed at an extremely oblique angle with the long axis of the canal, which runs forward and inward, and a longitudinal fracture through the roof of the canal, if

continued in the same direction, would rupture the membrane of Schrapnell, traverse the anterior portion of the tympanic cavity, and, avoiding the delicate structures of the middle ear, terminate somewhere in the angle between the petrous and the squamous portions of the bone—that is, at the point where the temporal bone articulates with the posterior angle of the great wing of the sphenoid bone. (Fig. 2.)

Fig. 2.

The probable route of the fracture terminating in the carotid canal at the margin of the foramen lacerum medium. The dotted circle shows the seat of the trephine hole.

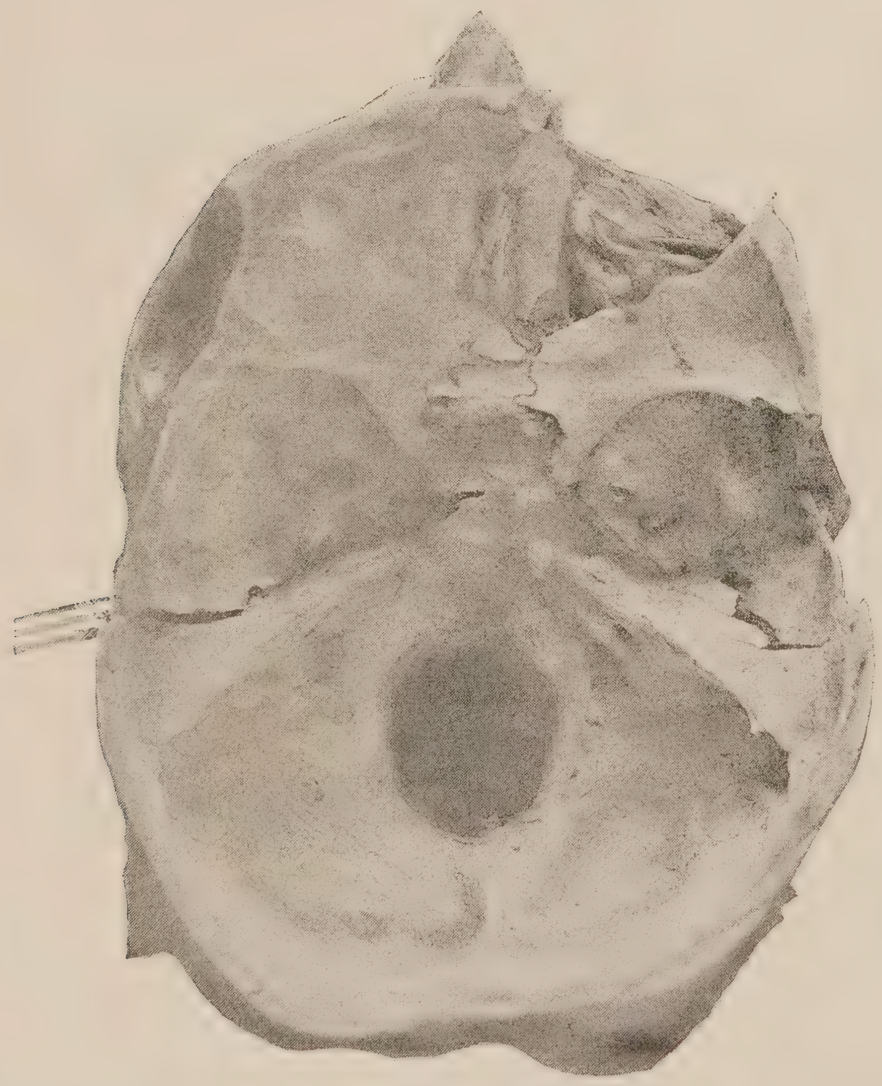
Experiments on the cadaver to show what kind of violence probably caused the lesion of our patient demonstrated that such fractures could be produced by blows with a blunt instrument applied directly above the external auditory meatus.

The head of a fresh cadaver was tested in this way in the autopsy-room and a number of dissecting-room subjects were also used for this purpose. In all cases the fracture ran down the anterior and external portions of the petrous bone to terminate either in the external margin of the foramen lacerum medium or the junction of the posterior angle of the great wing of the sphenoid with the temporal bone—that is, near the foramen spinosum through which the meningeal artery passes. In some cases the broken edges were closely in contact; occasionally the fissure was broad so that one could look down into the tympanic cavity where the bones of the ear could be seen still maintaining their normal positions. (Fig. 3.)

It seems highly probable, therefore, that this is the type of fracture ordinarily produced by blows received over the external ear. But in

the present case the bone was not only broken, but the brain was also severely lacerated and the dura ruptured. My explanation of the injury is as follows: In falling from his horse the patient struck upon the forehead producing the contusion already noted. This accounted, probably, largely for the hemorrhage from the nose and vomiting of blood. The horse in falling struck him a severe blow on the side of the head, producing the fracture of the temporal bone, and at the same time the weight of the animal pressing heavily upon the cranium, forced apart the edges of the fracture and forced out the cranial contents, rupturing the dura. As the bones sprang back into place the protruded brain was nipped and expelled through the ear, thus accounting for the narrow strip of brain found on the patient's coat-sleeve.

Fig. 3.



Fractures through the petrous bones into the foramen lacerum medium on each side, produced experimentally by blow of a mallet before opening the head. A glass rod is placed in the left external auditory canal.

The laceration of the brain extended deeply into the middle lobe, reaching as far as the first temporo-sphenoidal convolution and possibly

involving a portion of the inferior frontal convolution. The lesion must have involved the peripheral portions of the centre for speech, and the effects of the injury must have been felt as far as the centres for the face and tongue.

We have in this case an example of fracture of the temporal bone, involving the base and communicating with the external and middle ear, and possibly the Eustachian canal, but not opening directly into the vault of the pharynx. As a complication, we have laceration of the middle lobe and rupture of the posterior branch of the middle meningeal artery.

The symptoms of fracture of the base were characteristic, consisting of profound coma, bleeding from the ear, and the escape of brain-substance. The presence of cerebro-spinal fluid was not noticed at the time of the injury, owing to the free hemorrhage, and later was, naturally, not seen flowing from the ear, owing to the establishment of suitable drainage.

The escape of brain-tissue is not a common occurrence in fractures of the base, but when observed is regarded as an unfailing sign of that lesion. Von Bergmann[1] says: The escape of brain-substance from the fracture of the base is an undoubted symptom of this injury, and at the same time of laceration of the membranes; it may escape from the external ear or the nose.

Nancrede[2] says: The escape of brain-substance by the ear or nose, of course, definitely settles the fact of basal fracture, but is of rare occurrence.

The treatment of fractures of the base, until recently, has been largely expectant. Even long after antiseptic treatment of wounds had been established, surgeons do not appear to have realized that in this form of fracture they often had a compound fracture to deal with. Recent literature, however, has taken cognizance of the claims of this region to the benefits of antisepsis. Keen[3] advises disinfection of the ear, filling it with boracic acid, and covering the scalp with sublimate gauze. He recommends opening the roof of the orbit or nostril in penetrating wounds of those regions, to provide for free drainage. He also advises tamponing the nostrils with sublimate gauze to prevent passage of infective air through these passages.

Dennis[4] lays down rules for the treatment of this fracture. He thinks the entire scalp should be shaved and disinfected. He also irrigates the external auditory canal and the nasal passages. "Both of these should be rendered thoroughly aseptic, after which the ears should be packed with iodoform or bichloride gauze, and some absorbent cotton plugs placed in the nose."

[1] Deutsche Chirurgie, Lieferung, 30.
[2] International Encyclopædia of Surgery, vol. v.
[3] Handbook of Medical Science, vol. viii. p. 226.
[4] The Medical Record, November 23, 1889.

In the case reported here every effort was made to prevent infection through the Eustachian tube. The epistaxis and hematemesis in this case may have been due, in part, to the flow of blood from this canal. It was, in any event, important to keep the inner opening to the middle ear in as aseptic a condition as possible.

This can hardly be accomplished by gauze or cotton plugs in the nose unless they are brought in direct contact with the mouth of the Eustachian tube.

Rosenbach has shown that the pharynx is a not infrequent seat of the pyogenic cocci, and the antisepsis of this region must, consequently, also be carefully attended to.

Frequent douching of the naso-pharynx and mouth with antiseptic sprays is perhaps as efficient and safe a method as any. At the Boston City Hospital diphtheria patients are sprayed hourly in this way with a 1 : 1000 solution of sublimate given with an insufflator, and often without waking the patient. A little iodoform powder dusted once a day on to the membrane near the Eustachian opening may aid in preserving the region in an antiseptic condition. Should a more extensive fracture, such as one crossing the vault of the pharynx, exist, it might be advisable to plug the pharynx and posterior nares with antiseptic gauze. Such a dressing would be no annoyance to a comatose patient, and it is difficult to see how a fracture of this region could be dressed antiseptically in any other way.

The use of the trephine has been recommended for depressed fractures of the base, but has not been frequently employed, and then only in the occipital region. Hutchison[1] has applied the trephine successfully near the foramen magnum for a depressed fracture of that region in a boy fourteen years of age; puncture of the membrane was subsequently resorted to for a serous effusion, and recovery took place.

Keen recommends an opening for drainage in the roof of the orbit in punctured wounds at that point. Allis (*Annals of Surgery*, July, 1889) drilled through the cribriform plate for drainage of a compound depressed fracture of the frontal bone situated an inch above the right eye—a drainage tube three-eighths of an inch in diameter was passed through the openings. The patient recovered.

Trephining for drainage in fractures of the base is a device which I do not find mentioned in a moderate search through the literature of this subject, and its successful application to Dr. Homans's patient seems to me of sufficient importance alone to justify placing the case on record.

Applied just above the external auditory meatus the instrument makes an opening which drains most effectually the middle fossa. This operation would, therefore, be applicable not only to fractures of the temporal

[1] Medico-Chirurgical Transactions, vol. ii. p. 105.

bone but fractures through the sphenoid, and even those involving the anterior fossa. For this latter region, however, it would probably be desirable to place the trephine near the pterion, as injuries to the middle meningeal artery might thus also be attended to.

In fractures of the occipital bone the opening should be made below the insertion of the tentorium in order to drain the posterior fossa, and care should be taken to avoid the lateral and occipital sinuses. The trephine should, therefore, be placed below the superior curved line, and not too near the crest of the occiput.

Recently I had an opportunity to see two autopsies of cases of fracture of the base. In one case there was a partially detached fragment of the occiput half-way between the foramen magnum and superior curved line. The fragment could not be removed, owing to severe hemorrhage from the lateral sinus, and the surgeon had left the parts so as to favor drainage. On opening the skull but a small amount of extravasated blood was found in the pia mater of the cerebellum and medulla. In a second case where the sinuses had also been ruptured and no opening for drainage had been made, the entire surface of the brain was covered with extravasated blood.

In a fracture clearly involving the vault of the pharynx, even though it were not possible to say that the temporal bone had been injured, the trephine might be placed a little anterior to the auriculo-bregmatic line and a gauze drain inserted along the floor of the skull in the route of the fissured bone. A case can even be imagined where it would be proper to place the trephine upon the squamous portion of each temporal bone in order that the entire region of the sphenoid bone might thus be controlled by the drainage gauze.

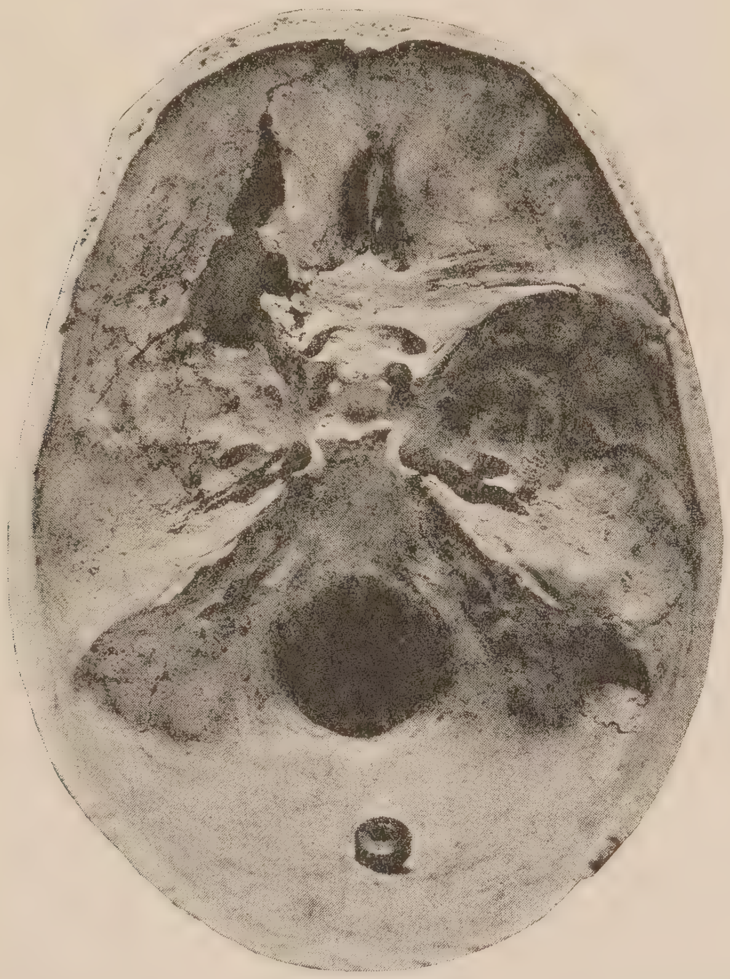
The materials used as drains in surgery of the brain should, it seems to me, differ essentially from those used ordinarily for this purpose elsewhere. The sharp edges of bone or rubber drains are sources of irritation and may even destroy brain substance by pressure. Catgut may be absorbed too soon, but otherwise works well. Horse-hair may be suited for the scalp, but could not easily be introduced into a lacerated wound of the brain-tissue without danger of irritation from the sharp points.

The antiseptic gauze used in my case seemed to meet all requirements. It can be easily slipped along the inner surface of the dura and constitutes not only a drain, but also an antiseptic dressing, which protects the cranial contents from invasion from the naso-pharyngeal cavities or the ear. It can drain, not only the wound, but may absorb from the subdural space fluid which accumulates on either side of the dressing, and by capillary attraction aspirate the exudation from different directions.

No better illustration of the advantages of drainage for fractures of

the base could be given than that afforded by the celebrated tamping iron case. (Fig. 4.) Dr. Harlow[1] thus describes the opening made through the floor of the skull:

FIG. 4.



Photograph of the base of the skull of the tamping iron case, showing the hole made by the iron in its passage through the frontal and sphenoid bones.

"The missile . . . entered the base of the skull at a point, the centre of which is one and one-fourth inches to the left of the median line, in the junction of the lesser wing of the sphenoid with the orbital process of the frontal bone, comminuting and removing the entire lesser wing with one-half of the greater wing of the sphenoid bone; also fracturing and carrying away a large portion of the orbital process of the frontal bone, leaving an opening in the base of the cranium, after the natural efforts at repair by the deposit of new bone, of one inch in its lateral by two inches in its antero-posterior diameter."

In his remarks upon the case he says:

"The bolt did little injury until it reached the floor of the cranium, when, at the same time that it did irreparable mischief, it opened up its way of escape, as without this opening in the base of the skull, for drainage, recovery would have been impossible."

[1] Passage of an iron bar through the head. Read before the Massachusetts Medical Society, June 31, 1868. John M. Harlow, M.D.

[Reprinted from Transactions of the Association of American Physicians, May, 1890.]

DEMONSTRATION OF PATHOLOGICAL SPECIMENS.

By HAROLD C. ERNST, M.D.,
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I. Bacilli of Leprosy.

Dr. Ernst showed a cover-glass preparation of the discharge from the ulcerations on the arm of a leper lately at quarantine in the city of Boston. The arrangement of the bacilli of leprosy in clumps was well demonstrated.

II. General Tuberculosis of Intestine after Inoculation of Milk from a Suspected Cow.

A rabbit was also shown with general tuberculosis of the intestines, resulting after the subcutaneous inoculation of a few drops of milk from a suspected cow. A child fed on this same milk was dead of tubercular meningitis, and a second—fed on the same milk—was showing the same symptoms. Bacilli of tuberculosis have been found in the lesions, and the post-mortem examination of the rabbit was made six weeks after the inoculation. This rabbit was one out of four inoculated—of which number, three showed the same appearances upon post-mortem examination.

III. Inoculation of "Contagium Vivum" of Vaccine Virus.

Dr. Ernst also communicated the preliminary results of work done by Dr. Stephen C. Martin, of Boston, in an attempt to isolate the actual "contagium vivum" of vaccine virus. These results, as far as tabulated, are as follows: (They are given precisely as Dr. Martin wrote them down. The numbers attached to the calves denote the number in the series of inoculations since the virus was first introduced into this country by the late Henry A. Martin, M.D.)

February	15,	1890.	Inoculated tube No. 1, from vaccine vesicle on calf 1461.
"	20,	"	Inoculated calf 1464 from tube No. 1 in two places, producing two vesicles, from which on
"	27,	"	Inoculated calf 1465 in three places, producing three good vesicles.
"	26,	"	Inoculated tubes No. 2 and W from No. 1.
March	5,	"	Inoculated tube No. 3 from No. 2.
"	13,	"	Inoculated calf 1467 in two places from No. 3, producing two vesicles, from which on
"	20,	"	Inoculated calf 1468 in five places, producing one fine vesicle, two fair, and two showed no reaction. From the first, on
"	27,	"	Inoculated tubes x, y, z, and Nelly Lahey, aged two and a half years; Wm. Burns, aged three years. Both had perfect and typical vaccinia. The aureola began at end of eighth day; decided reactionary fever; crust fell on twenty-second to twenty-fifth day. (On May 5th vaccinated Nellie Lahey again with vaccine of proved vigor. Inspected arm on the sixth day, and found effect to be absolutely *nil.*)
"	24,	"	Inoculated tubes 1a and 1b from No. 1, and 3a and 3b from No. 3.
"	25,	"	Inoculated tube 1a1 from 1a (third generation), and inoculated 3b1 from 3b (fifth generation).
"	27,	"	Inoculated calf 1469 from tubes 1, 3, W, 1a, and 1a1. Characteristic vaccinal effects in all.
April	9,	"	Inoculated tubes x1, y1, z1 from x, y, z, respectively.
"	24,	"	Inoculated calf 1474 in three places from tube No. 3. In two places from tube 3b1 (fifth generation). In two places from tube x1. In one place from tube z. Typical vesicles in one place from No. 3; in both places from No. 3b1. Slight vaccinal effects in the other three places (from x1 and z).
May	1,	"	Inoculated calf 1475 from the above. On the eighth day more or less typical vaccine vesicles in all. From which took points and vaccinated children on May 9th.

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The reasons for not giving a full account of the growth with which the experiments were conducted would seem to be very good ones. The work is not by any means yet completed, and there may be mistakes that later investigations will correct. Too much credit cannot, however, be given to Dr. Martin for the patience and conscientious application that he has brought to the work thus far.

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